THURSDAY, DECEMBER 30, 1880

## PERUVIAN BARK

*Peruvian Bark: a Popular Account of the Introduction of Chinchona Cultivation into British India.* By Clements R. Markham, C.B., F.R.S. 1860–1880. (London: John Murray, 1880.)

"THE enterprise undertaken by me in 1859 of introducing the cultivation of Peruvian bark trees into British India and Ceylon is now an assured success." With these words Mr. Markham begins his preface, and a perusal of the convenient history he has put together of the gradual steps by which during the past twenty years this success has been reached, enables us to fully share the satisfaction with which they must have been written. Not merely has a cheap supply of febrifuge alkaloids been brought within reach of the fever-haunted population of India, but a new and highly-profitable industry has been opened to the planters of our tropical colonies, and the yield of an inestimable drug placed beyond risk of exhaustion.

Enthusiasm is in most enterprises essential to success. If a certain tinge of impracticability often accompanies it a moderate experience of human nature disposes us to regard this with a good deal of toleration. We may as well confess at once therefore that the pleasure with which we have studied Mr. Markham's pages would have been greater but for his insistance throughout on two grievances, in neither of which do we find ourselves in any way persuaded by his advocacy. One of these—the other is more serious, and must be adverted to further on—is irritating in inverse proportion to its importance. The names of genera employed in systematic botany are Latinised forms, very arbitrary, and often, it must be allowed, unscholarly in their construction. But they are symbols or dockets under which scientific information can be arranged. If there is one thing about which botanists, of whatever nationality, are agreed, it is that the docket, having once been promulgated and brought into use, shall not be meddled with. It may be abolished or merged in some other, but being a mere symbol it cannot be tampered with without disturbing all kinds of mechanical aids to study, such as indexes and catalogues, and so adding to the worry of life. From a literary point of view the correction of *Cinchona* into *Chinchona* may be desirable, but the trouble of having two spellings in circulation is too great a price to pay for the mere satisfaction of literary propriety. It cannot be said therefore that this is merely a literary question like such spellings as those of diocess and chymistry affected by the *Times*, while from a technical point of view it has been already discussed and conclusively decided against Mr. Markham in the pages of this journal.

The genus *Cinchona*—as we must still beg leave to call it—includes all the plants at present known to yield quinine and allied alkaloids. It has rather more than thirty species, some of which however are medicinally valueless, while the rest vary individually in the amount and character of the alkaloids they yield. The native habitat of the genus is very restricted; it is only found on the Andes between 10° N. and 19° S. lat., and between 2500 and 9000 feet of elevation. Besides this the several species are closely limited to particular portions of the general area.

The native inhabitants seem to have set little store on the febrifugal properties of the cinchonas, and indeed to have been little aware of them except in the neighbourhood of Loxa, where a Jesuit was cured in 1600 of a fever at Malacotas by Peruvian bark, and to this day the local prejudice against its use is very strong. In 1638, however, the Countess of Chinchon, wife of the Viceroy of Peru, was cured of intermittent fever by bark sent by the Corregidor of Loxa. The remedy, whose reputation was now established, was carried by her to Spain in 1640, and became known as *Pulvis comitissæ*. In 1670 it was sent to Rome by the Jesuits and distributed to members of that order throughout Europe. Hence it came to be called Jesuit's bark, and it is interesting to find that its merits became accordingly a party question between Protestants and Catholics.

For more than a century (till 1776) the only bark met with in commerce was that brought from the neighbourhood of Loxa. This was called Quinquina, from the Indian name *quina-quina*, *quina* meaning bark, and the reduplication the possession of medicinal properties. The plant producing the bark was described by Linnæus under the name of *Cinchona officinalis*, to be rechristened afterwards by Humboldt and Bonpland *Cinchona condaminea*, a change correctly rejected by Mr. Markham, following Sir Joseph Hooker, and, be it remarked, on precisely the same grounds as those on which the rechristening of the genus as *Chinchona* must also be rejected.

As early as 1735 Ulloa represented to the Spanish Government that the Loxa forests could not long survive the reckless treatment to which even then they were subjected. And this was in spite of the intelligent efforts of the Jesuits, who endeavoured to enforce replanting as a religious duty. The Loxa bark, eventually distinguished as Crown bark from being reserved, when other kinds became known, for use in the Royal Pharmacy at Madrid, is represented in old collections of Materia Medica, such as that of the College of Physicians, by massive fragments which must have been detached from very old trees. Mr. Markham tells us that it is now only found in commerce in the minutest quills. As the Loxa bark became scarce the search after other supplies of cinchona bark was stimulated. The botanical expedition of Ruiz and Pavon sent by the Spanish Government in 1777 resulted in the discovery of seven species of *Cinchona*, yielding gre bark, near Huanuco in Northern Peru. Mutis, another Spanish botanist, believed that he first detected a *Cinchona* in Columbia in 1772, though a resident in Bogota challenged his claim to priority.

The well-known "red bark" of the slopes of Chimborazo seems to have been known early in the last century, and later to have found its way into European markets, though it was not till 1857 that the plant yielding it was clearly identified by Dr. Klotzsch. The yellow or Calisaya barks of Bolivia, first discovered by Haenke in 1776, did not become of commercial importance till 1820, when quinine, the most important active principle of Peruvian bark having been isolated by the French chemists, Pelletier and Caventou, yellow bark was recognised as richer in it than any other kind.

It is not necessary to follow in detail the interesting account given by Mr. Markham of the recklessness with which the natural supplies of Cinchona bark were drawn upon. The inconvenience of a precarious dependence upon the South American forests has for at least forty years occupied the attention of scientific men in Europe. Royle, in 1839, urged the introduction of Cinchonas into India, and pointed out the Nilgiri Hills as a suitable locality. The Dutch botanists had been no less urgent that the experiment should be made in Java, and Hasskarl was commissioned to proceed to Peru in 1852 to obtain seeds. In this he succeeded, but the bulk of the seeds eventually proved to belong to a species worthless medicinally, which was afterwards named *C. Pahudiana*. He also obtained, by the aid of a Bolivian named Henriquez, 400 plants of the yellow bark, *C. Calisaya*, only two of which unfortunately survived in Java. The mishaps of the Dutch enterprise cannot be followed here, instructive as they are to any one interested in the cultivation. A happy accident, to be presently alluded to, was a kind and well-deserved turn of fortune in its favour, and a greater measure of success than could ever have been hoped for now seems assured to it.

The Government of India in 1852 first proposed the introduction of cinchona into that country, and several abortive attempts to effect it were made with the aid of the Foreign Office, but without success. In 1859 Mr. Markham was officially employed by the present Lord Derby, who was then Secretary of State for India, to undertake a mission to South America for the purpose. His previous travels in the Cinchona region, though for ethnological and not for botanical inquiry, and his knowledge of the Spanish and Quichua languages singularly fitted him for the task. The plan laid down by him was extremely comprehensive, and has at last been fully carried out, or nearly so. It was nothing less than the introduction into India of all the species of *Cinchona* yielding bark of known commercial value. This plan was adopted as it was *à priori* uncertain which kinds would turn out best adapted for Indian cultivation, and it was desirable that all should be tried; it involved no less than five distinct expeditions to the different districts of the Andes already mentioned.

Mr. Markham visited himself in 1860 the yellow bark region in Southern Peru and Bolivia, accompanied by a young gardener named John Weir, recommended by Messrs. Veitch. The plants collected reached England in fifteen Wardian cases, but the heat of the Red Sea was fatal to them and they all eventually died. A supply of seed which Mr. Markham had arranged for at Caravaya arrived in India in 1865 and germinated satisfactorily.

Mr. Pritchett, who had travelled in the Huanuco district, was employed to make a collection of the grey bark plants, and to these also the Red Sea was fatal, but the loss again was retrieved by the safe transmission to India of seed which grew well. The red bark region was visited, at the suggestion of Sir William Hooker, by the well-known botanical traveller, Dr. Spruce, who was residing in South America at the time, and he was accompanied by Robert Cross, a Scotch gardener, recommended by the Kew authorities. The plants collected by Dr. Spruce were more fortunate, and reached India in good condition in 1861 under Mr. Cross's charge. This skilful collector then returned to South America and obtained the seed of the crown bark from the Loxa forests, which reached India in 1862 and germinated abundantly. Before returning to Europe he visited the Columbian forests in 1863 and secured seed of Pitayo bark (*C. Pitayensis*), which however had lost its vitality before it arrived in India. He was therefore sent again in 1868, and this time secured both plants and seeds, which were transmitted to India in a living state. The only remaining kinds of importance which had not been introduced into India were the Calisaya de Santa Fé, yielding soft Columbian bark, and *Cinchona cordifolia*, yielding hard Carthagena bark; to procure these Mr. Cross was despatched on another mission, from which he returned in 1878, bringing cuttings of both kinds, and these were successfully propagated at Kew, which had indeed in every case been made the depôt for the receipt of the successive consignments and their despatch to India. The Carthagena bark is now well established in India, Jamaica, and it is hoped in Ceylon. But the fate of the Calisaya de Santa Fé is still doubtful, as one consignment succumbed to the heat of the Red Sea, which is so great an obstacle to the transport of plants, intolerant of great heat, and no news as to the second instalment taken out in charge of Mr. Cross has yet reached this country.

We must but very briefly hurry over the interesting pages in which Mr. Markham describes what has been done in India. Red bark has everywhere taken the lead. Next to this, in the Nilgiris, crown bark has succeeded best; the other kinds have made but little progress. Unfortunately little care seems of late to have been taken in Southern India to keep the different kinds distinct, and as the species hybridise very freely it is not easy to say what some of the plants actually in cultivation precisely are. In the Himalayas, however, besides red and crown bark-plants, *C. Calisaya* (yellow bark) and *C. micrantha* (one of the species yielding grey bark) also do well.

The share taken by Kew in this important enterprise enabled the advantages secured by the Indian Government to be extended to other tropical possessions in the Empire. Sir William Hooker was allowed to transmit a share of the seeds and plants to Ceylon, Jamaica, Trinidad, Mauritius, and St. Helena. In the three latter islands the cultivation has made but little progress; in the first it is now one of the staple resources of the planters; while in Jamaica the crown and red bark bring in an annual revenue to the Government, which leaves an ample surplus after paying the whole expenses of the botanical department.

One of the most singular incidents in the whole story has still to be told. Mr. Charles Ledger, who had long resided in South America, hearing of Mr. Markham's enterprise, employed a native servant, Manuel Mamani, to collect seed of the best Calisaya or yellow bark tree. Four years elapsed before he succeeded, as each year the blossom of the trees was destroyed by frost. These seeds were transmitted to London to the care of Mr. Ledger's brother, and it is believed were offered to the Indian Government, who refused to purchase them. Half was eventually sold to the Dutch Government and half to Mr. Money, a planter on the Nilgiris. This fortunate purchase has put quite a

new face upon the cultivation in Java. The bark of some of the trees has yielded as much as 10 per cent. of quinine; and the news of this remarkable result has produced much the same effect on Cinchona planters in Ceylon and Southern India as the discovery of a gold-field on the inhabitants of an Australian city. The Java officials have however behaved with singular liberality in the matter, and in the course of a few years it cannot be doubted that Ceylon will be abundantly supplied with this valuable kind, which, there seems reason to think, may prove to be a distinct species. Part of the seed sent in the first instance to the Nilgiris seems to have found its way to Sikkim, and the Government plantations there are believed to be in possession of a strain of Calisaya, little if at all inferior to that possessed by the Dutch.

The Government of Bengal have effected an enormous saving by using, in hospitals and dispensaries, instead of quinine imported from Europe, the febrifuge manufactured at the Sikkim plantations. The Government estimated that in consequence, by the end of 1879, "the plantations will have cleared off the entire capital that has been invested in them."

And this leads us to what is really the painful feature in Mr. Markham's book. He complains in repeated and in bitter terms of the want of justice which has been shown to those whom he employed in the business of collecting. "Those who did the work have not received fair recompense for most valuable services." It is rather singular to find that he adduces in support of this statement the case of Mr. Ledger, who was not even in any way commissioned to do what he did. But the remuneration which his actual agents received was the ground of no complaint on their part, and was in point of fact liberal compared with that which is given to the collectors who are constantly employed by the great nurserymen, and who too often lose their lives in their arduous pursuits without the satisfaction of feeling that they are doing so in an enterprise like this of lasting utility. But we fear that if Mr. Markham's assistants have reason to complain the blame must, on his own showing, be laid at his own door. He tells us (p. 271): "The system I adopted was . . . to include very slight remuneration in the original agreements. *Thus the loss to Government would be insignificant if the work was not executed satisfactorily.* If, on the other hand, the arduous tasks were successfully performed . . . I anticipated no difficulty in obtaining fitting recognition for such distinguished services." We leave our readers to judge of the probability of such a scheme answering Mr. Markham's expectations. We may go further, and ask how the claims would have stood if, notwithstanding all the pains that were taken, the cultivation of Cinchonas had fared in India—as might even have happened—no better than it at first did in Java.

But there are many other things pleasanter than this which we should like to touch upon if this review had not already run to an inordinate length. So many Englishmen are now in one way or other interested in colonial industries that it will be strange if this interesting book does not find as many readers as it deserves. Besides a complete history of the Cinchona enterprise in the Old World, it gives, in an appendix, accounts of some other South American vegetable products, notably india-rubber. The steps taken at Mr. Markham's instance for the introduction into India of the most important rubber-yielding plants of the New World have been from time to time recorded in our pages. We have only to repair one inadvertent omission on Mr. Markham's part, and point out that the transmission of the Para rubber plant to India was secured by the exertions of Mr. Wickham, as recorded in the Kew Report for 1876, p. 8.

## *PRACTICAL BLOWPIPE ASSAYING*

*Practical Blowpipe Assaying.* By George Attwood. With Seventy-four Woodcuts. (London: Sampson Low, Marston, Searle, and Rivington, 1880.)

THIS book shows many signs of carelessness on the part of the author. At the very outset, in the Introduction, we meet with strange statements. Mr. Attwood divides the elements into those which are of commercial value and those which are of no commercial value. In the latter class we find Uranium and Tungsten; surely the author does not intend to deny the value of pitchblende and wolfram. He classifies zirconium among the non-metallic elements.

The first part of the work describes the reagents and apparatus; the second, we are told, contains the modes of determining any one of the sixty-four well-recognised elements, and in the third part we have the methods adopted by the author for making quantitative assays by the blowpipe. Finally, Part IV. contains some tables showing the English and American values of gold according to its fineness, and the value of gold coins in the United States.

The apparatus employed is much the same as that recommended by Plattner. Like Neumann, Mr. Attwood very wisely uses riders with his balance instead of the very small weights supplied by some of the other Freiberg opticians; but the balance would be improved by the addition of a movable arm for shifting these riders. The steelyard devised by the author will probably be of use to explorers. From practical experience with the batea I can fully endorse all that is said in its favour, but why are the merits of the iron pan ignored? It has the advantage that it will stand rougher usage than the batea. Again, for washing a sample of tin ore nothing will beat the Cornish vanning shovel.

I regret to see no mention of the useful little pastilles and crucibles made out of charcoal powder, proposed by Griffin thirty or forty years ago and adopted by Plattner. Col. Ross's aluminium plate for sublimates seems also to have escaped Mr. Attwood's notice.

With reference to the list of reagents I must remark that the author does not name all the reagents which his tests require, whilst others are inserted which he does not appear to put to any use. I should be glad to know what he means by inserting "nitrous acid" among his reagents. This is not a misprint for "nitric acid," because that acid has been already named.

The plan of the second part of the work is not one which I should recommend. It simply contains a list of tests for the various elements, but gives no systematic scheme for making the examination of an unknown substance. I fear that the "direct" method advocated by Mr. Attwood will often prove a very tedious one. Many of the tests themselves are not so complete as they

ought to be. In describing the tests for barium it is said that the bead "can be flamed," but no explanation is given of the process of flaming. The capital test for bismuth with potassium iodide and sulphur is entirely ignored.

I now come to the third part, which treats of quantitative assays. Mr. Attwood's plan of making a check assay in every case with a small quantity of the pure metal is certainly calculated to give the operator confidence in his results. The author adopts 1½ grain as the amount of ore to be taken for an assay. I think he would have done better to have followed Plattner and used the French weights, because there is less chance of making errors where each milligramme means 1 per cent.

For the silver assay Mr. Attwood employs pieces of ordinary charcoal instead of the far more convenient and portable charcoal crucibles designed by Plattner. He also describes a crucible assay for silver ores, which does not appear to possess any advantage over Plattner's scorification method.

There is one most unfortunate error in the book to which I feel bound to call attention. Mr. Attwood gives some tables for calculating the number of ounces of gold or silver per ton from the results of assays of 1½ grain of the ore. In an unlucky moment he forgot that gold and silver are weighed by *troy* weight, and calculated his tables for *avoirdupois* ounces. The consequence is that these tables are not only valueless, but also highly misleading. Let us take one case as an example. Suppose that 1½ grain of ore had yielded 0·01 grain of fine metal. We look down the table (p. 117), and find, according to Mr. Attwood, that the yield would be 238·93 oz. per ton; in reality the yield should be 217·77 oz.

Some neat little retorts have been designed by the author for distilling ores of mercury and amalgam, but he does not mention Küstel's assay.

On coming to the tin assay we have the peculiar statement that silica may be separated from tin ore by boiling t with hydrochloric acid. "The assay being finely powdered, the silica is dissolved." "The dissolved silica is decanted off" (p. 158). Cornish mine agents will be surprised when they are told that, in order to obtain correct results, it is necessary to wash or van as much as *5 lbs.* of an ordinary tin ore (p. 159).

Under the head of nickel no mention is made of the valuable ores from New Caledonia.

Small mistakes are numerous. The size of a box is said to be "twelve inches square" (p. 3); we note also: "*a* most useful addenda" (p. 24); "chloride of ammonia" (p. 33); "manganite" instead of manganate (p. 53), and permanganate (p. 54). The term "raw iron" is used frequently instead of "pig iron," and shows that the author has copied Cornwall's translation blindly. Coal, anthracite, and graphite are said to "volatilise" when heated in the platinum spoon (p. 82). Sieves are made with 2000 holes per "linear" inch (pp. 100 and 137). In the description of cupellation (p. 106) we read: "The lead parts with portions of its oxygen to the copper and other base metals."

In conclusion I think that the value of the book would be increased if a list of *errata et corrigenda* were inserted, correcting some of the errors which, I regret to say, impair its general usefulness. C. LE NEVE FOSTER

## OUR BOOK SHELF

*Über die von den Trichopterenlarven der Provinz Santa Catharina verfertigen Gehäuse.* Von Dr. Fritz Müller. *Archivos de Museu national.* Vol. iii. pp. 99-134, and 209-214. Rio de Janeiro, 1880. (Aus dem Portugieischen übersetzt von dem Bruder des Verfassers, Dr. Hermann Müller in Lippstadt.)

DR. FRITZ MULLER has for some years been engaged upon an investigation of the habits of the Caddis-flies of Santa Catharina, and has shown extraordinary skill in breeding these insects, a matter always difficult, and especially in the case of those that inhabit running water. The results of his researches were foreshadowed in various notes published in the *Zoologischer Anzeiger* and in the *Transactions* of the Entomological Society of London for 1879. But it was well known that the extended information and figures would be given in the Rio de Janeiro *Archivos*. As this publication is somewhat difficult to obtain, and as most of us are not familiar with Portuguese, Dr. Hermann Müller has conferred a great boon by publishing a translation of the paper (accompanied by the two folded plates) in the *Zeitschrift für wissenschaftliche Zoologie* for the present year (pp. 47-87, plates iv. and v.). It is needless to state that the details are of the greatest interest, and we have here the most important contribution to the natural history of *Trichoptera* that has appeared since the publication of Pictet's "Recherches" on the species of Geneva, and worked out in a far superior manner. We cannot here even allude to most of the many marvels of insect-architecture and habits that Dr. Fritz Müller has revealed. Some of the most interesting are the numerous forms of *Helicopsyche*, which build little sand-cases so like shells that they have been described as such; those *Dentalium*-like cases, originally noticed by Aug. St. Hilaire as *Grumicha*, which name our author retains; those instances of parasitism (or worse) in which a larva of one species dispossesses that of another of its house and converts it to its own purposes; those very numerous forms of *Hydroptilidæ*, the most minute of all *Trichoptera*, with cases of the most varied and wonderful structure; above all, that most interesting fact that the rain-water which collects at the bases of the leaves of some *Bromeliaceæ* has a special fauna of its own, including at least one Caddis-worm. The descriptions of these and many others will be read with delight by every biological student; and we hope Dr. Müller will follow up the paper by records of further discoveries, for here, as in all his works, the evidences of superior powers of observation strike one on every page.

The plates are excellent, and aid much in a realisation of the descriptive portion. Dr. Müller's artistic powers are so marked that we cannot but regret he has not furnished details of the form and structure of the perfect insects also, which would have greatly aided systematists; in fact the perfect insects are only alluded to in a casual manner.

*Voyages of the Elizabethan Seamen to America.* Thirteen Original Narratives from the Collection of Hakluyt, Selected and Edited, with Historical Notices, by E. J. Payne, M.A. (London: De La Rue and Co., 1880.)

WE do not quite understand Mr. Payne's reason for publishing this selection from Hakluyt's classical collection of voyages. The selection is, however, judicious, and cannot fail to be interesting, and at the same time instructive, to those who desire to become familiar with the first beginnings of English conquest in America. Mr. Payne's familiarity with the subject of British colonisation, as exemplified in his excellent little "History of European Colonies," specially qualifies him for making such a selection as the present. His brief Historical Introduction enables the reader to understand the special significance of the voyages contained in this volume. He

shows the various causes in operation at the time to instigate such voyages, causes mainly political and commercial. Other influences were however at work, not the least of which was "the total transformation which astronomy and geography had undergone" during the sixteenth century. The narratives here given are those of Hawkins's and Frobisher's three voyages, Drake's voyages of 1577 and 1585, Gilbert's voyage of 1583, Amadas and Barlow's voyage, 1584; Cavendish's first and last voyages, and Raleigh's voyage to Guiana. Prefixed to each narrative is a short historical introduction.

## LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

### Black Sheep

THE following extract of a letter from Mr. Sanderson of Chislehurst, who permits me to publish it, seems worth placing on record. It relates to the former frequent appearance of spotted or black sheep in the Australian flocks, as long as animals thus coloured were of use to man, although they were never, as far as Mr. Sanderson knows, separately bred from, and certainly not in his own case. On the other hand, as soon as coloured sheep ceased to be of use they were no longer allowed to grow up, and their numbers rapidly decreased. I have elsewhere assigned reasons for the belief that the occasional appearance of dark-coloured or piebald sheep is due to reversion to the primeval colouring of the species. This tendency to reversion appears to be most difficult quite to eradicate, and quickly to gain in strength if there is no selection. Mr. Sanderson writes:—"In the early days before fences were erected and when shepherds had charge of very large flocks (occasionally 4000 or 5000) it was important to have a few sheep easily noticed amongst the rest; and hence the value of a certain number of black or partly black sheep, so that coloured lambs were then carefully preserved. It was easy to count ten or a dozen such sheep in a flock, and when one was missing it was pretty safe to conclude that a good many had strayed with it, so that the shepherd really kept count of his flock by counting his speckled sheep. As fences were erected the flocks were made smaller, and the necessity for having these spotted sheep passed away. Their wool also being of small value the practice soon grew of killing them off as lambs, or so young that they had small chance of breeding, and it surprised me how at the end of my sheep-farming experience of about eight years the percentage of coloured lambs produced was so much smaller than at the beginning. As the quantity of coloured wool from Australia seems to have much diminished, the above experience would appear to be general."

CHARLES DARWIN

### The Nature of the Chemical Elements

DR. ARMSTRONG'S article in NATURE, vol. xxiii. p. 141, has brought to my mind some calculations I made more than a year ago to test a theory I had long previously entertained. Most of us who have paid much attention to the subject are agreed that the elements are capable, under exceptional circumstances, of profound chemical change. Mr. Lockyer is searching, with success as it appears, for contemporary evidence of this by examining the condition of the solar surface. The other line of evidence is historical, and turns mainly on the classification of the numerical values of chemical symbols. It is of course only with the latter that I have to deal.

The classifications proposed by Newlands and Mendelejeff are comprehensions of much similar preceding work. They appear to me to be faulty in two ways: (1) on account of the seriously large number of elements they wholly fail to include, and (2) because of the strong stress they lay upon arithmetical series of a rough *per saltum* character. As I do not know of any real case of *per saltum* chemical change, I do not think the elements should be classified on such a basis. What is wanted is a system capable of including—with exactness and not mere approximation—the whole of the elementary numbers; that system to be represented in the mathematical symbols of ordinary chemical change, and therefore free from a *per saltum* character. I have to a great extent succeeded in finding such a system, and the results of testing it at many points are as follow:—1. There is probably only one fundamental form of matter; and this, as has been previously supposed, yields our ordinary elements and many others by ordinary polymerisation. 2. Almost all the elementary numbers have been tried, and, with the exception of H and Cl, which are a little troublesome, they fall into order very exactly. 3. This order exhibits no discontinuity, and is similar to a case of ordinary chemical change. 4. There is clearly an upper limit to this order; in other words, elementary numbers of more than a certain magnitude appear to be impossible.

Sir B. C. Brodie's method is really a classificatory one; and I with others had been very desirous to read the Third Part of the Calculus, in which it was promised ampler play. It will be a matter for much regret if his premature death should have prevented this. But what he did publish was sound and sure: the first real symbols chemistry has yet enjoyed, and the only ones hitherto proposed whereby the process and the results of chemical change admit of unitary as well as kinetical representation.

EDMUND J. MILLS

### Smokeless London

AS I hope soon to have an opportunity of reading a paper on this subject before a scientific audience I need not occupy your valuable space by replying to your correspondents of last week in detail. I may say however that the scheme has been carried out in practice at a gas-work to which I shall afterwards refer. When it was found that the apparatus for making gas on an extraction of six hours was insufficient for supplying the wants of the long winter evenings the distillation was stopped when gas had been removed to the extent of 5000 cubic feet per ton. The larger quantities obtained from the coal per unit of time and the superior illuminating power obtained per unit of volume tided over the difficulty and rendered the existing plant sufficient. No practical obstacles were discovered in discharging the retorts. I do not think the difference between an extraction of 5000 and 3333 cubic feet per ton would make a material change in this respect. Mr. Mattieu Williams points out a much more serious obstruction in the plethoric indifference of the gas companies. In reply to E. R. F. I may say that the fuel resulting from a uniform extraction of 3333 cubic feet per ton is practically smokeless if it is taken hot from the retorts and immediately quenched with water.

Westminster, December 27 W. D. SCOTT-MONCRIEFF

### Colliery Explosions and Coal-Dust

ACCEPTING Mr. Galloway's view that in many mines the extent and destructiveness of colliery explosions are due to the distribution of coal-dust in the air, may I suggest the possibility of preventing the explosion from spreading beyond the sphere of the fire-damp by sprinkling the floors throughout, at certain regular intervals, with mineral oil? A shady road, with one such sprinkling, may be kept free from dust for several weeks during the summer, and the corridors of a mine, not being open to wind and rain, would of course remain wet for a longer period. A saucer filled with dust and treated with mineral oil will retain the oil for months even when exposed to sun and rain. The mixture of coal-dust and oil is quite uninflammable. The experiment may perhaps be worth trying in one of the drier coal-mines.

December 27 R. RUSSELL

### Geological Climates

PROF. DUNCAN is under the impression that the claim of *Araucaria Cunninghami* to have flourished at Bournemouth during the Eocene, rests on "a bit of a leafy part of a tree," and that this bit is "squashed." The foliage is however abundant there, occurring almost wherever vegetable remains are found, from the east of Bournemouth Pier to half a mile beyond Boscombe. In one place, where a bluff is literally full of it, the disarticulated branchlets are perfect, and not in the least degree compressed. Again, the determination was not made by Prof. Haughton, but rests upon my statement that this foliage and that of *A. Cunninghami* cannot be distinguished one from the other. That it is Araucarian foliage I am perfectly satisfied; but whether the existing Australian species is identical and unmodified, must remain doubtful until other

organs besides foliage are found, it being by no means absolutely certain that because the foliage is identical the species are so. The discussion raised by Prof. Haughton, and continued by Prof. Duncan and Mr. Wallace, seems therefore hardly worth prolonging, since it is based upon an assumption that is only probably correct. But even if the identity were proved, a single species is not satisfactory evidence of former temperature.

I am indebted to Mr. Winslow Jones for the only information that I have yet obtained about the growth of either species in England. He recollects a small tree of *A. excelsa*, growing near the water's edge in a garden on the upper portion of Falmouth Harbour, which he believes died three years ago. He has seen flourishing trees at Naples, Cintra, Malta, and Algiers, but even Northern Italy seems beyond the range of successful cultivation. Of the two *A. Cunninghami* seems the more tender, though possibly its less symmetric growth may have excluded it from many gardens. In Madeira it grows generally best close to the sea and in sheltered places.

Lindley was mistaken in regarding the two species as one. All the needle-leaved (Eutacta) section of Araucaria are certainly closely allied, for the species, however distinct in other respects, possess two kinds of foliage, that of the young plants being identical in all: yet otherwise the species are clearly and distinctly marked off from each other.

With further regard to the identification of the Bournemouth foliage with Araucaria, I find that Massalongo[1] gives an excellent photograph of the same foliage from Chiavon, in North Italy, and of an immature cone consisting of 250 scales. Although existing Sequoias have cones with from 16 to 20 scales, Schimper says: "Il est sans aucun doute un *Sequoia* et peut-être identique au *S. Sternbergii*. Les cônes ont la plus grande ressemblance avec ceux du *S. gigantea*" (Pal. Végétale," vol. iii. p. 573). I am beginning to lose all faith in the so-called science of palæo-botany as worked out by our Teutonic brethren. Not only is the above quotation an absurdity, for which Heer is responsible, but I fail to see any good evidence to support the change made by Heer from *Araucaria*[2] *Sternbergii* to *Sequoia Sternbergii*. The foliage is more Araucaria-like than Sequoia-like, and has been found associated with an Araucaria cone, but never with any Sequoia cones. It has nothing to do with the Icelandic foliage, neither with the Upper Miocene foliage from Turin, nor that from Bilin nor Oeningen. The true *Araucaria Sternbergii* characterises a well-marked horizon, that of the Newer Eocene or Oligocene in Central Europe, and has been found at Barton in Hampshire; it differs from the Middle Eocene form (*A. venetus*, Mass.) of England and Italy in the needle-like leaves hugging more closely to the branchlet, as the latter differs in its turn from the Araucaria of the Grès du Soissonnais, which has needles very widely opened out. This progressive change may have taken place *pari passu* with the changing climate. At Sheppey, where foliage is plentiful, I have met with a beautifully-preserved axis of an Araucaria cone with the basal scales attached, exactly as we find them in the existing species.

Now with regard to Mr. Wallace's letter, I pointed out in NATURE, vol. xix. p. 126, that the Tertiary fossil plants, even of the Eocene, require at most an increase in temperature of 20°, and that the land connection between Europe, Greenland, and America, which there is reason to suppose existed then, would, by shutting out Arctic currents, have produced more than the required increment. If this theory appeared for the first time in my article, however clumsily I may have worded it, and if it has been of use to Mr. Wallace, it is only fair that the fact should be acknowledged, while if it has escaped his notice he will perhaps pardon my now drawing his attention to it. At the same time the publication of the Tertiary flora of North-East Siberia, which I had not then seen, and of Saghalien, has modified the views I put forward in a manner which I trust I may shortly find time to explain.

J. STARKIE GARDNER

## Chalk

MR. WALLACE'S theory that chalk was deposited in comparatively shallow water requires careful examination before it is accepted by geologists. I do not think he has given sufficient evidence to bear out his views which are necessary to his theory of continents.

Mr. Wallace cites the resemblance between chalk and Globigerina-ooze, namely—

The similarity of the minute organisms found to compose a considerable portion of both deposits; several species of Globigerina appearing to be identical in the chalk and the modern Atlantic mud; the presence of Coccoliths and Discoliths in both formations; the abundance of Sponges in both; the presence of *Porifera vitrea*, the nearest representative of the Ventriculites of the white chalk; the resemblance of the forms of Echinoderms; and without attempting to reconcile these with a shallow sea-deposit, he proceeds to state the case on the other side. This consists of the difference in analysis between chalk and Globigerina-ooze, the former containing more carbonate of lime and less alumina, the presence of silica in the Globigerina-ooze being perhaps counterbalanced by the flints in the chalk. The greater proportion of alumina certainly points to different conditions, which Mr. Wallace considers to be that chalk is the very fine mud produced by the disintegration of coral-reefs, and mentions a deposit resembling chalk at Oahu in the Sandwich Islands and the deposit in several growing reefs, without however attempting to show that there is any probability that the remains found in these would bear any resemblance to the Sponges and Echinoderms of the chalk, or why we find no remains of these Cretaceous coral-reefs.

Mr. Wallace does not state in what the greater resemblance between chalk and Globigerina-ooze of shallow over deep water consists, but he looks on it as "weighty evidence."

Mr. Gwyn Jeffries, he says, finds all the Mollusca of the chalk to be shallow-water forms, many living at forty to fifty fathoms, some confined to still shallower waters, while deep-sea forms are absent. The late Dr. S. P. Woodward considered that Ammonites probably lived in water not over thirty fathoms; and these facts are as difficult to reconcile with Mr. Wallace's views that chalk was deposited in a sea of not over a few thousand feet as in a deeper sea.

The rareness of corals and absence of coralline beds of the age of the Lower or Upper Chalk is an important objection to the theory that chalk was deposited similarly to the Oahu chalk, the beds of Maestricht and Faxoe being above the chalk, and the former are not even conformable with it.

The point I think is still an open one, whether we shall accept Mr. Wallace's views that chalk was deposited in a comparatively shallow sea and not very far from land, or in a deep sea, the immense break between the chalk and Eocene beds giving ample time for very considerable alteration to have taken place in the disposition of land in the interval. I send this letter in the hope that a discussion on the point may elicit new facts bearing on the subject.

S. N. CARVALHO, JUN.

8, Inverness Terrace, Kensington Gardens, W.

## On Estimating the Height of Clouds by Photography and the Stereoscope

THE great practical value of meteorological science and the desirability of extending its usefulness by the collection of data relating to atmospheric current will perhaps be sufficient excuse for asking attention to anything likely to promote this end.

In studying the currents and other peculiarities of the atmosphere a method of estimating the height, motion, and character, as also the position with respect to each other, of each stratum of cloud, is a requirement of almost paramount importance, the value of the means employed being proportional to the number of particulars provided in its record, and the facility with which any set of observations can be compared to another at any future period. With such ever-changing subjects as clouds in constant motion, and having no strongly-defined marks, the use of theodolites is almost out of the question, and the sextant and mirror process for similar reasons would be a very tedious operation.

These considerations have induced me to endeavour to make use of photography and the stereoscope, the former to secure a couple of simultaneously-exposed photographs at the extremities of a base line, and the latter to observe them reproduced apparently solid for the respective distances of the points composing the picture to be measured when superimposed on a scale of distances and placed in it. The base line is thus practically reduced to the width of the eyes, and the difficulties arising from motion eliminated.

The recording apparatus consists of a base 50 or 100 feet long, constructed of wood and turning on a pivot at the centre of its length, its extremities being suitably supported by a framework of wood or other material upon which they could easily roll. The small cameras for the ends of this are each to be hinged at the back of its base to a second board having a graduated quadrant and rackwork erected from one of its sides for adjusting

[1] "Specimen photographicum." Verona, 1859. Plate xxi.
[2] Actually described as Araucarites, a useless modification in this instance.

the camera to any degree of altitude. These supplementary boards are then pivoted at the centre of part of a divided circle, previously inlaid in the wood at the extremities of the base line, in such a manner that a line passing through the axis of the lenses would cut the pivots. The cameras thus furnished can be adjusted with ease to any vertical or horizontal angle. These angular adjustments of the two instruments must always coincide, with the slight exception that the horizontal ones must make internal angles with the base included between them, or, in other words, the lenses of both require to be directed to a point opposite to the centre of the base line.

The cameras also require their rapid exposing shutters to be electrically connected, to ensure the pair of sensitive plates being impressed at the same instant, and each dark slide employed to have a fine wire strained at its centre from top to bottom immediately in front of the prepared plate, and as close as possible to it without touching. The transparent lines produced in the developed negatives by these wires will constitute the zero of distance of any pair, and during the operation of reading off must be made to agree with similar ones on the scale of measurements obtained as follows:—

Upon a large cardboard rule a number of squares in fine black lines, one inside the other, and each one slightly out of the centre of its predecessor to the right hand, the outside square being then divided with a line at a tenth part of its diameter to the left of its centre. This line will indicate the zero of the scale. After placing a distinguishing mark or number in the corner of every square for purposes of identification, the cardboard will be ready to be photographed and reduced at the same time to the intended size of the cloud negatives. Two transparent positives copied from this and observed when placed side by side in a suitable stereoscope with the edges representing the left-hand one of the cardboard together, will appear in that instrument with the lines composing the zero only a few inches away, and the squares as a succession of vertical planes commencing some distance from that and receding from the eye in the order of greater to less, each one representing its own distance in space.

To find the value of these distances it will be necessary to focus the two cameras upon some terrestrial objects whose distances can be measured by any of the known methods, and negatives taken. The two resulting landscapes, when placed in the stereoscope, each superimposed face to face upon its respective scale, and the fine vertical lines of the whole made to occupy one apparent distance, an operation offering but little difficulty, every object or point of the landscape will be found to stand out in the vertical plane suited to its own distance, the relation between them being noted for the values found by measurement of the one to be marked upon the other. As a scale prepared thus would be of no value for any other angle at which the cameras might be placed, it would be most convenient to make use of two or three angles only, more being quite unnecessary, and prepare a scale for each, or one with a reference table of values for the respective angles would suffice. Again, in respect of altitudes. As the terrestrial measurements would only be absolutely accurate for those of clouds in the zenith, or of them, if it were possible, from the earth's centre in any direction, the tables of reference would have to include calculated corrections for altitude, or the graduations could be valued for the most useful degrees by experimental means.

It will be gathered from the above that the constancy of length of the base line can be ascertained, and corrected if necessary, by taking a couple of views of the same landscape for comparison with the preceding pair; slight fluctuations of length would not however be of much consequence in dealing with the comparatively coarse measurements of thick masses of cloud floating in so short a distance as the few miles of atmosphere capable of forming them consists.

To ascertain the height of clouds photograph a pair of negatives, and place these in the stereoscope with a pair of scale plates agreeing with the angle at which they were taken, and adjust as for the landscapes described above. The data required may then be read off by noting the vertical place each stratum occupies.

Prints of these negatives should afterwards be made for the particulars of height, direction of motion of the respective layers, point of compass, wind rate, state of barometer, thermometer, and general remarks upon the weather, to be recorded upon them for comparison or circulation.

Meteorological observatories fitted with such an addition to their present splendid collection of instruments would have their powers of dealing with the atmosphere and weather changes greatly reinforced.

JOHN HARMER

Wick, near Arundel

### Correction of an Error in "Island Life"

MY friend Dr. Günther has kindly called my attention to an extraordinary error at p. 322–323 of my "Island Life," where I state that the Loch Killin Charr (*Salmo Killinensis*) inhabits a lake in Mayo County, Ireland; instead of a small lake in Inverness-shire, 2000 feet above the level of the sea, as given in Dr. Günther's original description in the *Proceedings* of the Zoological Society, 1865, p. 698. On referring to my MSS. notes for this part of my work, I find that the habitat was first correctly given, but subsequently scored out and altered to the erroneous Irish locality! Why this was done I cannot now discover; and I can only regret that I should have fallen into so palpable an error, and request such of the readers of NATURE as possess my book to make the necessary alterations.

ALFRED R. WALLACE

### Natural Science for Women

WILL you allow me to supplement your kindly reference to the instruction in physical science given to women in Bedford College, London, by the statements that for the last two sessions a class in biology has been conducted there by Mr. Charles Stewart of St. Thomas's Hospital Medical School. The course of study is in every sense a practical one, with special reference to the Preliminary Scientific and First B.Sc. examinations at the University of London, and the best testimonial to the excellence of the instruction in these various subjects is furnished by the remarkable success during the present year of the Bedford College pupils at the University examinations, a success not less marked in the Science than in the Arts examinations.

ALFRED W. BENNETT

### Movements of Leaves

A YEAR ago we had in our conservatory a healthy young plant of *Acacia mollissima*. It bore no flowers, but consisted of a simple axis adorned with the soft feathery leaves of its genus, which closed up at night. Our gardener however thought it would improve in appearance if it could be made to bear a few branches; and with that view he cut it back. His end was achieved: a new stem shot up from the section, and graceful limbs were thrown out in turn by it. But along with this a strange result followed: the fresh leaves borne by the new stem and by the branches now closed at night, while the old leaves below the section ceased to do so. These lower leaves have long since fallen off, but the upper ones kept to their habit, and at the present time all fold up at dusk save a few of the very oldest, which only partially shut, or, in one case, do not shut at all. When our plant was cut back it stood three feet high; now it stands seven: which shows that the vigour of the plant as a whole in no wise diminished by the operation.

Chislehurst, December 23

M. L. ROUSE

## *ON DUST, FOGS, AND CLOUDS*[1]

DUST, fogs, and clouds seem to have but little connection with each other, and we might think they could be better treated of under two separate and distinct heads. Yet I think we shall presently see that they are more closely related than might at first sight appear, and that dust is the germ of which fogs and clouds are the developed phenomena.

This was illustrated by an experiment in which steam was mixed with air in two large glass receivers; the one receiver was filled with common air, the other with air which had been carefully passed through a cotton-wool filter and all dust removed from it. In the unfiltered air the steam gave the usual and well-known cloudy form of condensation, while in the filtered air no cloudiness whatever appeared. The air remained supersaturated and perfectly transparent.

The difference in the behaviour of the steam in these two cases was explained by corresponding phenomena,

[1] Abstract of a paper read to the Royal Society of Edinburgh, December 20, by Mr. John Aitken. Furnished to NATURE by the Council of the Society.

in freezing, melting, and boiling. It was shown that particles of water vapour do not combine with each other to form a cloud-particle, but the vapour must have some solid or liquid body on which to condense. Vapour in pure air therefore remains uncondensed or super-saturated, while dust-particles in ordinary air form the nuclei on which the vapour condenses and forms fog or cloud-particles.

This represents an extremely dusty condition of the air, as every fog and cloud-particle was formerly represented by a dust-particle, which vapour by condensing upon it has made visible. When there is much dust in the air but little vapour condenses on each particle, and they become but little heavier, and easily float in the air. If there are few dust specks each gets more vapour, is heavier, and falls more quickly.

These experiments were repeated with an air-pump, a little water being placed in the receiver to saturate the air. The air was then cooled by slightly reducing the pressure. When this is done with unfiltered air a dense cloudiness fills the receiver, but when with pure air no fogging whatever takes place, there being no nuclei on which the condensation can take place. In this experiment, and in the one with steam, the number of cloud-particles is always in proportion to the dust present. When the air is nearly pure and only a few dust-particles present, then only a few cloud-particles form, and they are heavy and fall like fine rain.

The conclusions drawn from these experiments are: (1) that whenever water vapour condenses in the atmosphere it always does so on some solid nucleus; (2) that dust-particles in the air form the nuclei on which the vapour condenses; (3) that if there was no dust there would be no fogs, no clouds, no mists, and probably no rain, and that the supersaturated air would convert every object on the surface of the earth into a condenser on which it would deposit; (4) our breath when it becomes visible on a cold morning, and every puff of steam as it escapes into the air, show the impure and dusty condition of our atmosphere.

The source of the fine atmospheric dust was then referred to, and it was shown that anything that broke up matter into minute parts would contribute a share. The spray from the ocean, when dried and converted into fine dust, was shown to be an important source. Meteoric matter also probably contributed a proportion. Attention was then directed to the power of heat and combustion as a source of this fine dust.

It was shown that if there is much dust then each particle only gets a little vapour condensed upon it, that when the particles are numerous they become but little heavier, and easily float in the air, and give rise to that close-packed but light form of condensation which constitutes a fog, and therefore whatever increases the amount of dust in the air tends to increase fogs, and that when the dust-particles are not so numerous the cloud-particles are larger and settle down more quickly.

It was shown that by simply heating any substance, such as a piece of glass, iron, brass, &c., a cloud of dust was driven off, which, when carried along with pure air into the experimental receiver, gave rise to a dense fog when mixed with steam. So delicate is this test for dust that if we heat the one-hundredth of a grain of iron wire the dust driven off from it will give a distinct cloudiness in the experimental receiver, and if we take the wire out of the apparatus and so much as touch it with our fingers and again replace it, it will again be active as a cloud-producer. Many different substances were tried, and all were found to be active fog-producers. Common salt is perhaps one of the most active.

Heat, it is well known, destroys the motes in the air, and it might be thought that flame and other forms of combustion ought to give rise to a purer air. Such however is not the case. Gas was burned in a glass receiver, and supplied with filtered air for combustion, and it was found that the products of combustion of pure air and dustless gas gave rise to an intensely fog-producing atmosphere. It may be mentioned here that the fog-producing air from the heated glass, metals, and burning gas were each passed through the cotton-wool filter, and the air was in all cases made pure, and did not give rise to cloudiness when mixed with steam.

It will be seen that it is not the dust motes which are revealed to us by a beam of sunlight when shining into a darkened room, that form the nuclei of fog and cloud-particles, as these may be entirely removed by heat, and yet the air remain active as a cloud-producer. The heat would seem to break up the larger motes which reflect the light into smaller and invisible ones. When speaking of dust, it is to these infinitesimally small and invisible particles we refer. The larger motes which reflect the light will no doubt be active nuclei, but their number is too small to have any important effect.

It is suggested, and certain reasons are given for sup posing, that the blue colour of the sky is due to this fine dust.

Other experiments were made to test the fog-producing power of the air and gases from different sources. The air to be tested was introduced into the experimental receiver and mixed with steam, and the relative densities of the fog produced were noted. It was always found that the air of the laboratory where gas was burning gave a denser fog than the air outside, and that the air outside varied, giving less fog during wet than during dry weather. The products of combustion of gas burned in a Bunsen flame, a bright flame, and a smoky flame, were all tested and found to be about equally bad, and all much worse than the air in which they were burned. Products of combustion from a clear fire and from a smoky one gave about equal fogging, and both much worse than the air of the room.

Experiments were made by burning different substances. Common salt when burned in a fire or in alcohol flame gave an intensely fog-producing atmosphere, but burned sulphur was the most active substance experimented on. It gave rise to a fog so dense it was impossible to see through a thickness of 5 cm. of it.

The vapours of other substances than water were tested to see if they would condense in the cloud form without nuclei on which to deposit. All the substances experimented on, which included sulphuric acid, alcohol, benzole, and paraffin, only gave a cloudy condensation when mixed with ordinary unfiltered air, and remained perfectly clear when mixed with filtered air, all these acting like water vapour.

Before referring to fogs, which have now become so frequent and aggravated in our large towns, it was pointed out that caution was necessary in applying the results of the experiments.

The conditions of a laboratory experiment are so different, and on so small a scale, that it is not safe to carry their teaching to the utmost limits and apply them to the processes which go on in nature. We may, however, look to the experiments for facts from which to reason, and for processes which will enable us to understand the grander workings of nature.

It having been shown that vapour, by condensing on the dust-particles in the air, gives rise to a fogging, the density of which depends on the amount of fine dust in the air; the more dust the finer are the fog-particles, and the longer they remain suspended in the air. It having been also shown that all forms of combustion, perfect and imperfect, are producers of fog nuclei, it is concluded that it is hopeless to expect that, adopting more perfect forms of combustion than those at present in use, we shall thereby diminish the frequency, persistency, or density of our town fogs. More perfect combustion will, however, remove the pea-soup character from the fogs

and make them purer and whiter, by preventing the smoke which at present mixes with our town fogs and aggravates their character, and prevents them dissolving when they enter our rooms. Smoke descends during a fog, because the smoke particles are good radiators, and soon get cooled and form nuclei on which the water vapour condenses. The smoke thus becomes heavier and falls. This explains why falling smoke is often a sign of coming rain. It indicates a saturated condition of the atmosphere.

Sulphur when burned has been shown to be an intensely active fog-producer. Calculation shows that there are more than 200 tons of sulphur burned with the coal every winter day in London, a quantity so enormous as quite to account for the density of the London fogs. It is suggested that some restriction ought to be put on the amount of sulphur in the coal used in towns.

Before utterly condemning the smoke and the sulphur, it was pointed out that it would be necessary thoroughly to investigate and fully to consider the value of smoke as a deodoriser, and also the powerful antiseptic properties of the sulphurous acid formed by the burning sulphur. The air during fogs is still and stagnant. There is no current to clear away the foul smells and deadly germs that float in the air, which might be more deadly than they are, were it not for the suspended soot and burned sulphur. We must therefore be on our guard lest we substitute a great and hidden danger for an evident but less evil.

## *ON THE SPECTRUM OF CARBON*

ALTHOUGH fifteen years have passed since the possibility of one substance possessing more than one spectrum was first suggested by Plücker and Hittorf, the question of the existence of double spectra cannot yet be considered as decided. One of the elements to which multiple spectra have been attributed is carbon, which was at one time supposed to possess four different spectra: of these one has been shown to be due to oxide of manganese, a second to oxides of carbon, the origin of a third (obtained only from oxides of carbon) has hardly been discussed (though it may prove to be one of the true carbon spectra), and the other "carbon" spectrum—the best known of all—is the one first attributed to carbon by Attfield, but ascribed to acetylene by Ångström.

In a paper read before the Royal Society, and of which an abstract is given in NATURE, vol. xxii. p. 620, Professors Liveing and Dewar describe experiments to prove that this spectrum is that of a hydrocarbon, and not of carbon itself; and also that certain blue bands, best seen in the flame-spectrum of cyanogen, are due to compounds of carbon and nitrogen, and not to carbon itself. They attribute to hydrocarbon (amongst others) the yellowish-green group, which we will call $\gamma$, of wave-lengths from about 5635 to 5478, and the emerald-green group, which we will call $\delta$, of wave-lengths from about 5165 to 5082; and they attribute to nitro-carbon the two blue groups of wave-lengths 4600 to 4502 and 4220 to 4158, which we will call $\theta$ and $\zeta$ respectively.

As these result are directly opposed to my own experience, I have thought it necessary to repeat two of the experiments described in my paper on the carbon spectra in the *Philosophical Magazine* for October, 1869, under such conditions as to exclude (as far as lay in my power) all trace of hydrogen in the one case, and of nitrogen in the other.

The difficulty of supposing carbon to be present in the state of vapour at any temperature which we can command seems to be the chief reason why so many investigators think it necessary to attribute the spectrum in question (with experimental evidence or without it) to compounds of carbon. I am not aware that Ångström ever gave any experimental proof of his assertion that this spectrum was caused by acetylene.

On the other hand, the evidence that the spectrum is due to carbon is that first stated by Attfield, that if these lines "are absent in flames in which carbon is absent, and present in flames in which carbon is present," if they are "observable equally in the flame of the oxide, sulphide, and nitride as well as in the hydride of carbon," and if "present whether the incandescence be produced by the chemical force, as in burning jets of the gases in the open air or by the electric force, as when hermetically-sealed tubes of the gases are exposed to the discharge of a powerful induction-coil," then they "must be due to incandescent carbon vapour"; and if this is borne out by experiment the conclusion that the lines are due to carbon (as gas, liquid or solid) cannot be resisted, whatever may be the apparent impossibility of volatilising or even liquifying carbon, even by the most powerful current of electricity directed through it.

We must bear in mind how very small a quantity of a gas is often sufficient to give us a spectrum, and when the carbon spectrum is obtained by the decomposition of olefiant gas or cyanogen by passing sparks through the gas, the carbon certainly exists as gas in the compound which is decomposed, and before the liberated atoms unite together to form the molecules of the solid, there is surely no impossibility in their existing for the moment as gas—as gaseous carbon.

On an examination of Professors Liveing and Dewar's paper to ascertain the experimental evidence upon which the bands $\gamma$ and $\delta$ are attributed to hydrocarbon and not to carbon itself, we find it stated that "the green and blue bands characteristic of the hydrocarbon flame seem to be always present in the arcs, whatever the atmosphere. This is what we should expect if they be due, as Angström and Thalèn suppose, to acetylene, for the carbon electrodes always contain, even when they have been long heated in chlorine, a notable quantity of hydrogen."

Since then it is impossible to completely expel hydrogen from the carbon-poles, we must reject all the experiments in which the electric arc was observed in atmospheres of different gases, although "the green and blue hydrocarbon bands were seen more or less in all of them."

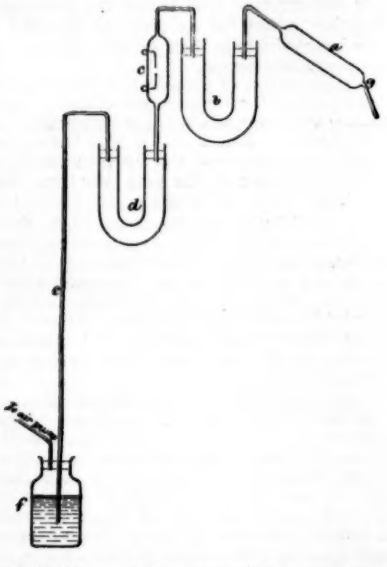
Turning then to other methods of producing the spectrum, we find it stated that in the flame of carefully-dried cyanogen "the hydrocarbon bands were almost entirely absent" (they should have been *entirely* absent); "only the brightest green band was seen, and that faintly." Hence we are to infer, I suppose, that the bands $\gamma$ and $\delta$, so brilliant in the flame of cyanogen in air or oxygen, are due to the accidental presence of hydrogen (see the extract from Morren's paper, NATURE, vol. xxii. p. 7. Dibbits also speaks of this spectrum as "by far the most magnificent" he has seen).

Next we have the experiment of burning hydrocyanic acid, in which, as we have hydrogen present, we expect to find the hydrocarbon bands brilliantly developed. But we find the result stated as "very much the same as that of cyanogen." The flames of hydrogen and sulphide of carbon, and of hydrogen and carbonic oxide, do not give the hydrocarbon bands (their spectra being continuous); a mixture of hydrogen and carbon tetrachloride gives them faintly, and a mixture of hydrogen and chloroform gives them strongly.

In all this we have no *proof* of the point in question, nor even any special probability that the bands are due to hydrocarbon; and yet, in the face of experiments in which the spectrum is obtained from cyanogen, when care has been taken to exclude hydrogen, we are asked to attribute the bands to the hydrocarbon formed by combination with some trace of hydrogen (as water or otherwise), supposed to be present as impurity. In the same way the presence of the bands $\theta$ and $\zeta$ obtained under circumstances when nitrogen has been intentionally excluded, is to be explained by "the extreme difficulty of

removing the last traces of air." So that in the case of cyanogen with a trace of hydrogen present, the spark persists in giving us the spectrum of hydrocarbon; and when we have naphthalin with a trace of nitrogen present, it gives us the spectrum of nitrocarbon!

Attfield states that the spectrum in question is obtained from *pure dry* cyanogen. "The ignition of the gases having been effected in air, it was conceivable that hydrogen, nitrogen, or oxygen had influenced the phenomena. To eliminate this possible source of error the experiments were repeated out of contact with air. A thin glass tube one inch in diameter and three inches long, with platinum wires fused into its sides and its ends prolonged by glass quills having a capillary bore, was filled with *pure dry* cyanogen, and the greater portion of this gas then removed by a good air-pump. Another tube was similarly prepared with olefiant gas. The platinum wires in these tubes were then so connected with each other that the electric discharge from a powerful induction-coil could pass through both at the same time. On now observing the spectra of these two lights in the simultaneous manner previously described, the




characteristic lines of the hydrocarbon spectrum were found to be rigidly continued in that of the nitrocarbon. Moreover, by the same method of simultaneous observation the spectrum of each of these electric flames, as they may be termed, was compared with the corresponding chemical flames, that is with the oxyhydrocarbon and oxynitrocarbon jets of gas burning in air. The characteristic lines were present in every case."

"The spectrum under investigation having then been obtained in one case when only carbon and hydrogen were present, and in another when all elements but carbon and nitrogen were absent, furnishes to my mind sufficient evidence that the spectrum is that of carbon."

Morren also adopted this method of producing the spectrum by taking the spark of an induction coil in a sufficiently rapid current of pure cyanogen at atmospheric pressure.

I have again repeated this experiment with cyanogen under conditions which would seem to ensure that the gas should be dry (see also *Phil. Mag.*, 1875).

The cyanogen was prepared by heating pure cyanide of mercury, which was finely powdered and placed in a piece of combustion-tubing (*a*) drawn out at both ends. In this it was repeatedly heated to the temperature of incipient decomposition whilst a current of dry air was drawn over it. One end of the tube was then closed by fusion at the point *g*, and the other bent round and fitted, as shown in the figure, to a U-tube (*b*) containing phosphoric anhydride—the discharge-tube *c* was interposed between this U-tube and a second U-tube *d* also containing phosphoric anhydride, the other branch of which was connected to one end of a vertical tube *e* of more than thirty inches in length, the lower end of which passed into mercury contained in the bottle *f*, the upper portion of which could be exhausted by means of the air-pump. The connections with the U-tube were made by means of perforated india-rubber stoppers, and the joints were surrounded during the experiment by melted paraffin.

The apparatus having been exhausted, the mercuric cyanide was heated till the apparatus was filled with cyanogen at atmospheric pressure; it was then again exhausted and again filled with cyanogen. After having been thus exhausted and re-filled five or six times, the spectrum of the spark between the wires at *c* was examined at various pressures. The spectrum figured in my paper in the *Philosophical Magazine* for October, 1869, was obtained, the groups $\gamma$ and $\delta$, with which alone we are at present concerned, being the brightest in the whole spectrum. Next careful search was made for the red hydrogen line. The cross-wires of a one-prism spectroscope were accurately adjusted to the red line, as seen in a hydrogen vacuum tube, and the spectroscope was then directed upon the spark in the cyanogen. No trace of the line could be observed.

A second experiment was devoted to the examination of the spark in an atmosphere of naphthalin vapour, from which nitrogen had been excluded as far as possible, in order to ascertain whether the bands $\zeta$ and $\theta$, which Professors Liveing and Dewar attribute to cyanogen, would be produced. Professors Liveing and Dewar are somewhat in error in saying that I laid much stress on the occurrence of these bands in carbonic oxide. They were never obtained very brilliantly from carbonic oxide (except under pressure), but they are obtained brilliantly from a naphthalin vacuum tube. I have obtained them also from a vacuum tube containing pure marsh-gas (my note-book remarks "$\theta$ very bright"), and as confirmation by an independent observer, I would remark that Plücker maps them in the spectrum of a vacuum tube containing *methyl*.

The vacuum tube in this second experiment contained pure solid naphthalin fused on the sides of the tube; this was placed in position so that the upper end passed through one hole in an india-rubber stopper into a flask filled with carbon dioxide; a vertical tube of thirty inches length passed through the second hole in the stopper of the flask, and its lower end dipped below mercury. The whole of the vacuum tube except the lowest portion was surrounded by a wider tube containing melted paraffin.

When the apparatus had been arranged, the experiment was commenced by passing a rapid current of carbonic acid through the vacuum tube, so as to fill the flask and escape through the mercury. After passing the gas for a considerable time, the lower end of the tube was closed by fusion, the naphthalin all melted down into this end, where it was made to boil violently, while the paraffin was maintained at a temperature of about 220° C. After the current of naphthalin vapour had lasted some time, the upper end of the tube was closed by fusion, the tube removed and cooled, and its spectrum examined. It gave a spectrum in which the groups $\zeta$ and $\theta$ were plainly seen.

It is to be hoped that some independent observer will repeat these experiments, so as finally to settle the question of the origin of these bands of what I must still call the "carbon" spectrum.

W. M. WATTS

## THE INDO-CHINESE AND OCEANIC RACES—TYPES AND AFFINITIES

I.

THE ethnological area here under consideration comprises the south-eastern corner of the Asiatic mainland, and nearly the whole of the Indian and Pacific Oceans. Of the three great divisions of the human family—the black, yellow, and fair—the two former alone are usually supposed to be represented in this region, the black by the Australians, extinct Tasmanians, Melanesians or Papûans, and Negritos, the yellow by the Indo-Chinese (Annamese, Siamese, Burmese, &c.), of the mainland, and the so-called "Malayo-Polynesians" of Oceanica. But it will be one of the main objects of these papers to show that room must here be henceforth made for the third also, and that most of the difficulties associated with the mutual classification of the other two are due to the omission or neglect of this third factor in the problem. It has long been an accepted doctrine of ethnologists that this fair or Caucasian type, using the term "Caucasian" in Blumenbach's sense, is limited by some mysterious law of nature or providential arrangement to the western portion of Asia, to the northern section of its African, and to nearly the whole of its European peninsula. But anthropology is a very young science, and as facts accumulate and knowledge expands, many of its conclusions too hastily arrived at will have to be modified or abandoned. The time seems to have already arrived for very materially modifying the views hitherto entertained regarding the geographical limits of the Caucasian species, which, instead of being confined to a western corner of the Old World, will be found to have been diffused in prehistoric times eastwards to within 2,500 miles of the American continent.

But the acceptance or rejection of this new doctrine will of course depend largely on the various senses in which the terms type, species, race, are understood by the different monogenist and polygenist schools. For the orthodox monogenist these words can obviously have but a relative meaning, for if all are necessarily sprung of one created pair, all have also necessarily become differentiated into the now existing types, these types thus sinking to the category of mere varieties. But to polygenists of all shades such expressions may naturally convey an absolute sense, the fundamental species now existing having presumably been evolved in so many independent centres, and for these the only question will be in *how many* centres? Yet even they cannot consistently base their theory on the eternal fixity of species, for they are all of them otherwise, and necessarily believers in evolution. They must therefore admit the abstract possibility of such comparatively slight transformism as the development of the dark from the yellow, the fair from either, lank from woolly hair, dolichocephaly from brachycephaly, the tall stature of the Tehuelch Patagonian from the pygmy Akka, or the reverse of all these processes. They may say that, assuming independent development from various anthropoids, such transformism is unnecessary to account for the present state of things; but they can never deny its inherent possibility, for it still remains a very trivial modification compared with the evolution of any given human from any given anthropoid type. Nor will they deny that in general differentiations of this sort are far more easy and explicable than independent growths, which involve so much more fundamentally radical changes. Consequently unorthodox monogenism, that is monogenism not starting from a created pair, but from one evolutionary centre, seems more rational and philosophic than any conceivable form of polygenism. This view seems in other respects to harmonise best with the actual conditions, and an effort has accordingly been made to give it expression in the subjoined definition of species, which differs in some important respects from those hitherto proposed: *Species is an aggregate of units resembling each other in all salient points, producing offspring of the same type in the same surroundings, or of continuously modified type in continuously modified surroundings, and themselves evolved of previous species similarly modified indefinitely.* Thus any given species or race (terms practically identical when used with scientific precision) exists only for the time being, is not and cannot be permanent, for it has become what it is by slow modification under slowly modified outward conditions, has had a beginning, may have an end. The best vindication of this truth is the geological record, which can only be explained either with Cuvier by the unwarranted assumption of successive fresh creations, or with common sense by regarding type or species as relative, not absolute concepts. Between the two views there seems to be no logical middle term.

It is therefore in this relative sense only that race or species are here to be understood, and in this sense it will be seen that all the three most fundamental types of mankind have existed from the remotest times in the wide area above defined. With their diverse modifications and intercrossings these three types form altogether seven main groups, which it will be convenient to take *seriatim* in the order adopted in the subjoined

*General Scheme of Indo-Chinese and Oceanic Races*

A.—DARK TYPES

I. NEGRITOS: Aetas; Andamanese; Samangs; Kalangs; Karons.

II. PAPÛANS:
- Central branch—Papûans Proper.
- Western branch—Sub Papûans West (so-called "Alfuros").
- Eastern branch—Sub-Papûans East (Melanesians).

III. AUSTRAL: Australians; Tasmanians (?)

B.—CAUCASIAN TYPE (Fair and Brown)

IV. CONTINENTAL BRANCH: Khmêr or Cambojan Group.

V. OCEANIC BRANCH: Indonesian and Sawaiori or Eastern Polynesian Groups.

C.—MONGOLIAN TYPE (Yellow and Olive Brown)

VI. CONTINENTAL BRANCH: Indo-Chinese Group.

VII. OCEANIC BRANCH: Malayan Groups.

### A—DARK TYPES

#### I. THE NEGRITOS: *Aetas; Andamanese; Samangs; Kalangs; Karons*

Of the three divisions of this type shown in our scheme the Negrito is probably the most primitive. It seems to have formed the aboriginal element in South-East Asia and Malaysia at a time when the Archipelago was still connected with the mainland; but it is now represented only in a fragmentary way by the wild tribes in the Philippines collectively known as Aetas, Aitas, or Itas, the so-called "Mincopies" of the Andaman Islands, the little-known Samangs of Malacca, probably the Karus or Karons[1] of the Arfak Hills behind Geelvink Bay, New Guinea, and a few surviving members of the Kalangs of East Java. From a number of specimens recently brought to Europe, the osteology of the Aetas and Andamanese has been carefully studied, the former by Virchow in Germany, the latter by Prof. Flower in England, with parallel and in many respects identical results. Virchow[2] describes the Aetas as "a brachycephalous race differing altogether from the Papuans and Australian Negroes, and no less so from the African Negroes." He adds that they are "strongly prognathous," the profile of some

[1] Described by M. Raffray ("Tour du Monde," April 26, 1879) as essentially distinct from the Papûans. "Ce ne sont pas des Papous, mais bien des Negritos, plus semblables aux sauvages aborigènes des Philippines qu'aux Papous Mélanésiens qui les entourent."

[2] In "Correspondenz-Blatt der deutschen Gesellschaft für Anthropologie," &c., 1872, p. 58.

crania consequently presenting an almost "orang-utan physiognomy." So also Prof. Flower[1] tells us that the Andamanese cranium is "as distinct as possible" from the Melanesian, and on all the available evidence he seems disposed to regard these islanders as "representing an infantile, undeveloped or primitive form of the type from which the African Negroes on the one hand, and the Melanesians on the other . . . . may have sprung." The relations of the Negritos to the Papûans, long a vexed question in anthropology, may thus be regarded as finally settled by the most competent authorities. One

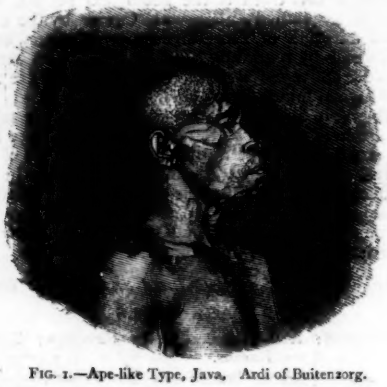


FIG. 1.—Ape-like Type, Java, Ardi of Buitenzorg.

doubtless, originally, they must now be regarded as two distinct species in the relative sense involved in our definition of that term. C. Staniland Wake also points out another important feature in which the two races differ. The Papûans proper, and especially the Melanesians of Fiji, New Caledonians and Solomon Islanders, are frequently furnished with well-developed beards, whereas the Andamanese and all other true Negritos, are absolutely beardless. "The absence of the beard seems to be characteristic of all the Negrito peoples, and this trait may in my opinion be safely added to the con-

FIG. 2.—Andamanese Type. Mourning Head-dress.

FIG. 3.—Australian Type. Woman in Mourning.

clusions of de Quatrefages touching the small black races of the Archipelago."[2]

The ape-like appearance of the Aetas, already spoken of by de la Gironnière, and now insisted on by Virchow, receives a startling illustration from the accompanying portrait (Fig. 1) of a Javanese Kalang named Ardi,

[1] In paper "On the Osteology and Affinities of the Natives of the Andamanese Islands," in *Journal* of Anthropological Institute, November, 1879, pp. 132-3.

[2] *La barbe considérée comme caractère de races*, in *Rev. d'Anthrop.*, an. 15, 1880.

recently if not still employed as a workman in the famous Buitenzorg (Sans-Souci) Botanic Gardens near Batavia. Here he was seen by C. B. H. von Rosenberg in 1871, and reproduced at p. 569, vol. iii. of that naturalist's work on the "Malay Archipelago" from an original photograph by van Musschenbroek, which has also been

FIG. 4.—Full-blood Papûan Type. North-west Coast New Guinea.

figured on an enlarged scale in Dr. A. B. Meyer's monograph on the "Kalangs of Java." Notwithstanding its startling ape-like appearance all doubt as to the correctness of the portrait is removed by the independent testimony of von Rosenberg and van Musschenbroek, the latter of whom informs me through Prof. Veth of Leyden

FIG. 5.—Full-blood Papûan Type. North-west Coast New Guinea.

(Letter, October 16, 1880), that "he has met with the same type in other parts of Java, though not so pronounced, and that it could always be traced to a Kalang origin." He adds that "this race is fading away and that the intermixture with Common Javanese has become

such that in most instances only faint traces of the peculiar type have been left." Meyer agrees with van Musschenbroek in regarding the Kalangs as a remnant of the aborigines of Java, possibly allied to the other

FIGS. 6, 7.—Malayo-Papûan Mixed Types. Body-guard of the Sultan of Ternate.

Negrito peoples of the Archipelago, and "occupying Java before it was peopled by the Malays." Ardi had come from the eastern parts of the island, where a few still linger no longer as a distinct tribe, but dispersed, like Ardi himself, amongst the general population. Hence

FIG. 8.—Melanesian Type. Vanikoro Chief.

the reader will doubtless be glad to have this authentic specimen of perhaps the very lowest type of mankind, now all but extinct.

Our next illustration (Fig. 2) is that of an Andamanese Negrito in a mourning head-dress, from a photograph sent to Europe by Mr. Man, and originally published in the *Anthropological Journal*, vol. vii. (1877) p. 416. It presents a singular resemblance to an Australian woman (Fig. 3) also in mourning, reproduced in the same place

FIG. 9.—A Motu Youth.

from a picture in Angus' "South Australia Illustrated" (plate 51).

The Negrito and Hottentot hair is usually described as growing in separate woolly tufts, or, as Topinard puts it, "in little peppercorn masses, separated by bald spaces." In his "Genealogical Classification of the Human

FIG. 10.—Maori Type.

Races and Languages" Venzel Krizhek revives the well-known classification of Friedrich Müller which makes this feature the basis of one of the main divisions of mankind, including the Hottentots, Papûans, and Negritos. Yet the phenomenon has absolutely no existence in nature. But such is the tenacity of errors of this sort that

it seems impossible to dispel the delusion, although, as Prof. Flower well remarks, "the report of a committee of the Paris Anthropological Society on the growth of the hair of a Negro in one of the hospitals of that city, published last year (1879) in the *Bulletin* of the Society, ought to set the question at rest for ever." It is curious that evolutionists should have discovered in man a trait which is characteristic of none of the anthropoids.

The Negritos, whether those described by Jagor and Meyer in the Philippines, or those visited by E. H. Man in the Andaman group, are all alike socially on the lowest level. They are all nomadic, though not pastoral, moving about from hill to hill, from coast to river-bank, in search of food or shelter from the weather or their enemies. They live on the fruits and roots of the tropical woodlands, on wild honey, snakes, frogs, fish, or such game as their feeble weapons (mostly spear and bow and arrow) are able to procure them. Yet, although indolent and incapable of providing for the future, they do not lack intelligence, for their brain capacity (index No. 74) is still immensely greater than that of the highest anthropoid ape. The Aetas often acquire a knowledge of the neighbouring Tagalog and Bisayan (Malayan) dialects, and the speech of the Andamanese seems from Man's specimens to belong to a highly agglutinating type. They appear to have no shrines or idols of any sort, in this greatly differing from the Papuans, and their religious thought is limited to a blind awe or fear of the powers of nature, for them doubtless supernatural manifestations. But our knowledge of their inner life is still far too restricted to pronounce very positively on these points. The Negritos are not generally suspected of cannibalism; but the Karons of New Guinea are certainly addicted to the practice. One of them, although quite a youth, admitted to M. Achille Raffray that he had already eaten fifteen men, treating it as quite a matter of course. They appear, however, to confine themselves to the bodies of their enemies slain in battle, and do not regard every stranger as so much "meat," like the Negroes of the Lualaba-Congo.

II. THE PAPÛANS: *Papûans proper; Sub-Papûans West ("Alfuros"); Sub-Papûans East (Melanesians)*

The Papûan domain is entirely oceanic, stretching in its widest sense from the island of Floris, Malaysia, eastwards to Fiji (120° – 180° E. long.), and from about the equator southwards to New Caledonia, at this point approaching the Tropic of Capricorn. In our scheme are shown three branches, a central, western, and eastern, which grouping has the convenience of being at once geographical, and to a large extent ethnical. The type itself, so named from the Malay word ريبوه (*papûwah* = frizzly), denoting one of its most striking characteristics, retains everywhere a considerable degree of uniformity in all essentials. But it is largely mixed with two distinct elements, the Malay in the west, the brown Polynesian or Sawaiori in the east. No doubt there are mixtures in New Guinea or the central region also, and notably on the south-east coast, to which the brown Polynesians seem to have penetrated in more recent times. But on the whole the bulk of the New Guinea people, including the adjacent Aru, Waigiu, Salwatty, Mysol, and Ké islanders, may be taken as the most typical branch of the race. The western division, composed of Malayo-Papûans, and often vaguely spoken of as "Alfuros," but whom I name Sub-Papûans West, comprises the Malaysian islands of Floris, Jilolo, Ceram, Buro, Goram, Timor, Wetter, Timor Laut, and neighbouring islets, without prejudice to the question of Papûan blood in Borneo and Celêbes. The eastern division, composed mainly of Sawaiori-Papûans, and whom I name Sub-Papûans East, comprises all the South Pacific Islands grouped as Melanesia. This term, Melanesia, referring to the prevailing black colour of the natives, is in every way convenient, so that Sub-Papûan East and Melanesian may be taken as practically synonymous. Here the chief groups are the Admiralty, New Britain, New Ireland, Solomon, New Hebrides, New Caledonia, Fiji, and it is to be noted that there are some, possibly many, Melanesians who betray no trace of mixture with the brown Polynesians, and who must consequently be regarded as pure Papûans. Such are the Vanicoro and Mallicolo people in the New Hebrides, and especially the Kai Colos of Viti Levu in Fiji, some specimens of whose crania Prof. Flower has recently shown to be absolutely the most dolichocephalous on the globe. As brachycephaly is a distinctive mark of the Negrito, so dolichocephaly is of the Papûan type. Consequently on this easternmost verge of the Papûan area we would seem to have, as far as is known, the very purest specimen of the race. This harmonises with the view I have ventured elsewhere to express, that the type was developed in a now submerged South Pacific Continent, moving westwards with the gradual subsidence of the land. For a long way east of New Guinea and North-East Australia, in fact quite as far as Samoa, the water is very shallow, averaging probably not more than 500 fathoms.

The accompanying illustrations may be taken as typical specimens of the three great divisions of the Papûan family. Characteristic full-blood Papûan types are those of two members of the Wosaoni tribe, North-West Coast of New Guinea (Figs. 4 and 5), from portraits by M. Raffray, originally figured in the *Tour du Monde*, for April, 1879, p. 267. In Figs. 6 and 7 we have good specimens of the so-called "Alfuros," or mixed Malayo-Papûans of the Archipelago, from sketches by M. Rosenberg, reproduced in his "Malay Archipelago," vol. ii. p. 401. The Vanikoro chief (Fig. 8), from Stanford's "Australasia," p. 476, represents a pure Melanesian head, extremely narrow and high, with long straight, but somewhat broad (platyrhine) nose and frizzly hair. In this front view the prognathism and dolichocephaly are of course not so perceptible as they would be in profile. The Motu youth (Fig. 9), from Stone's work, "A Few Months in New Guinea" (Sampson Low and Co.), illustrates the sub-Papûan East type, the moppy head being thoroughly Papûan, while the broad face, implying brachycephaly, must be referred to Sawaiori influences. The Motu people occupy a strip of about sixty miles on the south-east coast of New Guinea about Port Moresby, and speak a language of the Sawaiori type, apparently more allied to Samoan than to Malay. O. C. Stone's statement that they reckon up to one million must be received with caution, for the Samoans themselves cannot get beyond 10,000, while the Malays draw the line at 100,000. The familiar Maori (New Zealand) head (Fig. 10), from Stanford's "Australasia," p. 565, seems to support the now generally accepted view that the Maoris are not pure brown Polynesians, but a mixture of Rarotongans (Sawaiori stock) and Melanesians, the former predominating. According to some of their traditions on their arrival, probably some 600 years ago, they found the islands occupied by an aboriginal people, who must have been Melanesians, and who were partly exterminated and partly absorbed.

In point of culture the Papûans take a far higher place not only than the Negritoes and Australians, but even than most of the African Negroes. They build houses preferably on piles, cultivate the land with great care and intelligence, are everywhere settled in fixed tribal communities governed by well-understood usages. Alfred R. Wallace, a careful observer of this race, ranks them intellectually higher even than the Malays, accounting for their social inferiority by their less favourable surroundings and remoteness from the civilising influences of more highly-cultured peoples. A very pleasing account is given by Cook of his visit to the New Caledonians, who are generally regarded as an unfavourable branch of the

family. He describes the land about the villages as "finely cultivated, being laid out in sugar-canes, plantations, yams, and other roots, and watered by little rills conducted by art from the main stream, whose source was in the hills. . . . Some roots were baking on a fire in an earthern jar which would have held six or eight gallons; nor did we doubt its being their own manufacture." And further on: "The plantations were laid out out with great judgment, and cultivated with much labour." The reference to eartheuware is curious, because the Polynesians are generally supposed to be ignorant of the potter's art. But a taste for art in general, and especially for decoration, is one of the most distinguishing features of the Papûans. Their arms, idols, houses, boats, and other objects are often adorned with very tasteful and elaborate designs, and some of their tatooing presents extremely elegant patterns. They have domesticated the pig, dog, and poultry, and they cultivate the yam, sweet potato, banana, sugar-cane, taro, bread-fruit, and mango. Amongst their arms, besides the spear and bow, are the bamboo blowpipe, and flint knives and axes like those of the neolithic age in Europe. Cannibalism seems to be extremely rare in the West and in New Guinea, but until suppressed was universal in New Zealand and Fiji, and is still prevalent in New Britain and many other parts of Melanesia. From this division of the family it seems to have passed to the brown Polynesians, many of whom were formerly addicted to the practice. It reached its climax in Fiji when, shortly before the annexation of these islands to Great Britain, a whole tribe was condemned to be roasted alive and eaten. As they were too numerous to be consumed at one meal, it was arranged that at the annual taro harvest one family should be baked and eaten with that esculent, and the arrangement was scrupulously carried out until the annexation seasonably intervened to save a remnant of the tribe (*De Rici*).

A. H. KEANE

(*To be continued.*)

## *PROF. HUXLEY ON EVOLUTION*

AT the meeting of the Zoological Society on December 14, among the papers read was one by Prof. Huxley on the application of the laws of evolution to the arrangement of the vertebrata, and more particularly of the mammalia.

We take the following report of the paper from the *Times*:—

Prof. Huxley began by saying:—There is evidence, the value of which has not been disputed, and which, in my judgment, amounts to proof, that, between the commencement of the Tertiary epoch and the present time, the group of the Equidæ has been represented by a series of forms, of which the oldest is that which departs least from the general type of structure of the higher mammalia, while the latest is that which most widely differs from that type. In fact, the earliest known equine animal possesses four complete sub-equal digits on the fore-foot, three on the hind-foot; the ulna is complete and distinct from the radius; the fibula is complete and distinct from the tibia; there are forty-four teeth, the full number of canines being present, and the cheek-teeth having short crowns with simple patterns and early-formed roots. The latest, on the other hand, has only one complete digit on each foot, the rest being represented by rudiments; the ulna is reduced and partially ankylosed with the radius; the fibula is still more reduced and partially ankylosed with the tibia; the canine teeth are partially or completely suppressed in the females; the first cheek-teeth usually remain undeveloped, and when they appear are very small; the other cheek-teeth have long crowns, with highly complicated patterns and late-formed roots. The Equidæ of intermediate ages exhibit intermediate characters. With respect to the interpretation of these facts, two hypotheses, and only two, appear to be imaginable. The one assumes that these successive forms of equine animals have come into existence independently of one another. The other assumes that they are the result of the gradual modification undergone by the successive members of a continuous line of ancestry. As I am not aware that any zoologist maintains the first hypothesis, I do not feel called upon to discuss it. The adoption of the second, however, is equivalent to the acceptance of the doctrine of evolution so far as horses are concerned, and, in the absence of evidence to the contrary, I shall suppose that it is accepted.

Since the commencement of the Eocene epoch, the animals which constitute the family of the Equidæ have undergone processes of modification of three kinds: (1) there has been an excess of development of one part of the oldest form over another; (2) certain parts have undergone complete or partial suppression; (3) parts originally distinct have coalesced. Employing the term "law" simply in the sense of a general statement of facts ascertained by observation, I shall speak of these three processes by which the Eohippus form has passed into Equus as the expression of a three-fold law of evolution. It is of profound interest to remark that this law, or generalised statement of the nature of the ancestral evolution of the horse, is precisely the same as that which formulates the process of individual development in animals generally, from the period at which the broad characters of the group to which an animal belongs are discernible onwards. After a mammalian embryo, for example, has taken on its general mammalian characters, its further progress towards its special form is effected by the excessive growth of one part in relation to another, by the arrest or suppression of parts already formed, and by the coalescence of parts primarily distinct. This coincidence of the laws of ancestral and individual development, creates a strong confidence in the general validity of the former, and a belief that we may safely employ it in reasoning deductively from the known to the unknown. The astronomer who has determined three places of a new planet calculates its place at any epoch, however remote; and, if the law of evolution is to be depended upon, the zoologist who knows a certain length of the course of that evolution in any given case, may with equal justice reason backwards to the earlier, but unknown stages. Applying this method to the case of the horse, I do not see that there is any reason to doubt that the Eocene Equidæ were preceded by Mesozoic forms, which differed from Eohippus in the same way as Eohippus differs from Equus. And thus we are ultimately led to conceive of a first form of the equine series, which, if the law is of general validity, must need have been provided with five sub-equal digits on each plantigrade foot, with complete, sub-equal antebrachial and crural bones, with clavicles, and with, at fewest, forty-four teeth, the cheek-teeth having short crowns and simple-ridged or tuberculated patterns. Moreover, since Marsh's investigations have shown that the older forms of any given mammalian group have less-developed cerebral hemispheres than the later, there is a *primâ facie* probability that this primordial hippoid had a low form of brain. Further, since the existing horse has a diffuse allantoic placentation, the primary form could not have presented a higher, and may have possessed a lower, condition of the various modes by which the fœtus derives nourishment from the parent. Such an animal as this, however, would find no place in any of our systems of classification of the mammalia. It would come nearest to the Lemuroidea and the Insectivora, though the non-prehensile pes would separate it from the former, and the placentation from the latter group.

A natural classification is one which associates together all those forms which are closely allied, and separates them from the rest. But, whether in the ordinary sense of the word "alliance," or in its purely morphological sense, it is impossible to imagine a group of animals more closely

allied than our primordial hippoids are with their descendants. Yet, according to existing arrangements, the ancestors would have to be placed in one order of the class of mammalia and their descendants in another. It may be suggested that it might be as well to wait until the primordial hippoid is discovered before discussing the difficulties which will be created by its appearance. But the truth is that that problem is already pressing in another shape. Numerous "lemurs," with marked ungulate characters, are being discovered in the older Tertiaries of the United States and elsewhere; and no one can study the more ancient mammals with which we are already acquainted without being constantly struck with the insectivorous characters which they present. In fact, there is nothing in the definition of either Primates, Carnivores, or Ungulates, which affords any means of deciding whether a given fossil skeleton, with skull, teeth, and limbs almost complete, ought to be ranged with the Lemurs, the Insectivores, the Carnivores, or the Ungulates.

In whatever order of mammals a sufficiently long series of forms has come to light, they illustrate the three-fold law of evolution as clearly, though perhaps not so strikingly, as the equine series does. Carnivores, Artiodactyles, and Perisso-sodactyles all tend, as we trace them back through the Tertiary epoch, towards less modified forms which will fit into none of the recognised orders, but come closer to the Insectivora than to any other. It would, however, be most inconvenient and misleading to term these primordial forms Insectivora, the mammals so-called being themselves more or less specialised modifications of the same common type, and only, in a partial and limited sense, representatives of that type. The root of the matter appears to me to be that the palæontological facts which have come to light in the course of the last ten or fifteen years have completely broken down existing taxonomical conceptions, and that the attempts to construct fresh classifications upon the old model are necesssarily futile. The Cuvieran method, which all modern classifiers have followed, has been of immense value in leading to the close investigation and the clear statement of the anatomical characters of animals. But its principle, the association into sharp logical categories defined by such characters, was sapped when Von Baer showed that, in estimating the likenesses and unlikenesses of animals, development must be fully taken into account; and if the importance of individual development is admitted, that of ancestral development necessarily follows. If the end of all zoological classification is a clear and concise expression of the morphological resemblances and differences of animals, then all such resemblances must have a taxonomic value. But they fall under three heads: (1) those of adult individuals; (2) those of successive stages of embryological development or individual evolution; (3) those of successive stages of the evolution of the species, or ancestral evolution. An arrangement is "natural," that is, logically justifiable, exactly in so far as it expresses the relations of likenesses and unlikenesses enumerated under these heads. Hence, in attempting to classify the Mammalia, we must take into account not only their adult and embryogenetic characters, but their morphological relations, in so far as the several forms represent different stages of evolution. And thus, just as the persistent antagonism of Cuvier and his school to the essence of Lamark's teachings (imperfect and objectionable as these often were in their accidents) turns out to have been a reactionary mistake, so Cuvier's no less definite repudiation of the principle of Bonnet's "*échelle des êtres*" was no less unfortunate. The existence of a "scala animantium," is a necessary consequence of the doctrine of evolution, and its establishment constitutes, I believe, the foundation of scientific taxonomy. Many years ago, in my lectures at the Royal College of Surgeons, I particularly insisted on the central position of the Insectivora among the higher Mammalia; and further study of this order and of the Rodentia has only strengthened my conviction that any one who is acquainted with the range of variation of structure in these groups possesses the key to every peculiarity which is met with in the Primates, the Carnivora, and the Ungulata. Given the common plan of the Insectivora and of the Rodentia, and granting that the modifications of the structure of the limbs, of the brain, and of the alimentary and reproductive viscera which occur among them may exist and accumulate elsewhere, and the derivation of all Eutheria from animals which, except for their diffuse placentation, would be Insectivores, is a simple deduction from the law of evolution. I venture to express a confident expectation that investigation into the mammalian fauna of the Mesozoic epoch will, sooner or later, fill up the blanks which at present exist in the "scala mammalium." Prof. Huxley proceeded to give details on which his conclusions were based, and dwelt on the fact that much further careful work is needed to clear up problems before us.

## NOTES

We are enabled through the courtesy of the Council of the Royal Society of Edinburgh to present our readers with an abstract of a remarkable paper by Mr. John Aitken, on Dust, Fog and Mist. The paper opens up new lines of inquiry, and indeed a new future, to what has hitherto been one of the most difficult branches of meteorology, viz. the investigation of the vapour of the atmosphere, which we may safely predict meteorologists will not be slow in following up. Mr. Aitken continues the prosecution of the inquiry, and we learn that last week he has experimented with temperatures as low as 14°·0 F. with the result that equally as at higher temperatures, there is no cloudy condensation when there is no dust; but, when there is dust, cloudy condensation takes place on the dust nuclei, the amount of cloudiness being of course relatively small at such low temperatures on account of the small amount of vapour present. Taken along with Prof. Lister's experiments, in which it was shown that a single drop of rain developed organisms in sensitive solutions which would otherwise have remained for months unaltered, it shows that germ-producing matter, or germs themselves, form at least a part of the cloud- and fog-producing dust. Hence a cotton-wool respirator may prove a protection against disease. We have said enough to show that the paper is one of interest, not only to the physicist and the meteorologist, but also (and perhaps even specially) to the physiologist and the sanitarian.

We are pleased to learn that Dr. W. De La Rue, F.R.S., has been chosen a Corresponding Member of the Paris Academy of Sciences in the Section of Astronomy.

Baron de Chaudoir, Mr. McLachlan, and Baron C. R. Osten-Sacken have been elected honorary members of the Entomological Society of Belgium, filling the vacancies in the list caused by the deaths of Dr. Boisduval, M. Mulsant, and Dr. Snellen van Vollenhoven.

It is proposed to hold a meeting of the Association for the Improvement of Geometrical Teaching on Friday, January 7, in the Botanical Theatre of University College, Gower Street, at 11 a.m. The sub-committees appointed January 11, 1878, have prepared, and circulated amongst the members, draft syllabuses of solid geometry, higher plane geometry, and geometrical conics, and will present their Reports at the meeting. All persons interested in the elementary teaching of geometry are invited to attend.

According to a resolution of the St. Petersburg Society of Naturalists, the work of Prof. Wagner on "Medusæ and

Hydroids of the White Sea," will be published in German and French, with fifty tables of engravings.

THE Peabody Academy of Science (Salem, Massachusetts, U.S.A.), after a forced suspension of its publications for six years, announces that the *Memoirs* will be resumed at an early date.

DR. HOEK of Leiden writes us that a first part of the Zoological Results of the Dutch Arctic Cruises with the 60-ton Schooner *Willem Barents* will shortly be published. These results—a preliminary report of which Mr. D'Urban has given in the October number of the *Ann. and Mag. of Nat. Hist.*—will be published as an extra volume of the *Niederländisches Archiv. für Zoologie* (Leiden, E. T. Brill). The different articles will be written in English, French, or German, and the distribution of the material has been as follows:—Sponges, Dr. G. C. J. Vosmaer; Echinoderms, Prof. C. K. Hoffmann; Hydroids and Polyzoa, Dr. W. J. Vigelius; Nemertineans, Dr. A. A. W. Hubrecht; other Worms, Dr. R. Horst; Pycnogonids and Crustaceans, Dr. P. P. C. Hoek; Lamellibranchiate Mollusks, Mr. D. van Haren Noman; Gastropodous Mollusks, Mr. Th. W. van Lidth de Jeude; Fishes, Dr. A. A. W. Hubrecht; Birds, Prof. H. Schlegel. The first part contains the Worms, the Pycnogonids, the Lamellibranchiate Mollusks, the Fishes, and a description of the only mammal captured, and will be issued before the end of January.

THE death is announced of Prof. Karl B. Heller of the K.K. Theresianum at Vienna, a naturalist well known by his numerous writings.

REPORTS from Honolulu describe an eruption of the Mauna Loa Volcano (Hawaï) as the grandest which has ever been observed. It began on November 5 at some nine kilometres distance from the summit of the crater. The eruption of lava was accompanied by terrible explosions.

EARTHQUAKES are reported (1) from Brescia, where a shock was observed on December 10 in the afternoon; (2) from Schloss Trakostyan and environs (in the mountains of Northern Croatia), where three violent shocks occurred in the night of December 10-11; (3) from Smyrna, where, on December 12 at 9.40 p.m., a tolerably powerful shock was noticed. On the 23rd inst., about 5 p.m., a shock of earthquake was felt at Bucharest, Rustchuk, Kustendje, Galatz, Berlad, and Jassy. In the night of December 16–17 two earthquake shocks were felt in Agram, in close succession, about 11 p m. About the same hour shocks were observed in various parts of Carniola and Styria, *e.g.* in Gurkfeld at 11.4 and 11.9 p.m., in Grossontag, near Friedau, three quickly-successive shocks; in Pragerhof two pretty sharp shocks; in Peltau and in Marburg one strong shock each. In Csakathurn (Hungary) and neighbourhood strong earthquake motions were likewise observed the same night about 11.20 p.m. In the night of December 21-22 shocks were again felt in Agram, of which one about 1 a.m. was pretty violent. In the environs of Agram slight earth-vibrations are still constantly being experienced. At about ten minutes past five o'clock p.m. on December 25 two rather severe shocks of earthquake occurred at Odessa within a very short interval of each other. They appear to have come from the direction of the Middle Danube, and, passing through Roumania and Bessarabia, spent themselves here on the shores of the Black Sea in South Russia. They seem to have been most strongly felt at the Bessarabian towns of Bieletz, Kishineff, and Tiraspol, for the walls of some of the houses were cracked in consequence. At Odessa the effects were limited to buildings and furniture being more or less roughly shaken, or light articles such as vases, bottles, and glasses, being thrown down. The weather was extremely mild and calm at the time, and the sky but very partially clouded.

FALB's theory is gaining in favour with the population, especially as he predicted fresh earthquakes in the Agram region from December 15 to 31. Falb has enunciated his theory in a newly-published popular work entitled "Die Umwälzungen im Weltall" (Revolutions in the Universe). These are treated under three heads: (1) in the star regions; (2) in the region of clouds; and (3) in the depths of the earth.

THE tomb of Immanuel Kant at Königsberg will soon be decorated in a worthy manner. Upon a suitable pedestal a marble bust of the great philosopher will be placed. The bust is the work of Prof. Siemering.

"ALLERLEI gesammelte ornithologische Beobachtungen" is the title of a new book from the pen of Rudolf, Crown Prince of Austria, just published in a limited number of copies, which have been presented by the author to his friends.

A MONUMENT of the celebrated ornithologist Naumann was recently unveiled in the Schlossgarten at Köthen upon the occasion of the centenary of Naumann's birth.

THE German Fisheries Union have, according to the proposal of Prof. Nitsche of Tharand, resolved to offer a prize of 500 marks (25*l.*) for the best treatise on the following subject:—Of the ova of fish which are sown out for breeding, and particularly of the ova of the Salmonidæ, a large percentage is completely destroyed by fungi, well-known to pisciculturists as byssus or "mould," and belonging partly to the family of Schizomycetæ and partly to that of Saprolegniaceæ. A detailed botanical description of the respective genera and species, their biology and propagation, as well as an account of the manner of their introduction into the piscicultural apparatus, of the conditions which favour their development and of the way in which they destroy the ovum, is now required. At the same time the questions are to be discussed whether and by what means it would be possible to prevent their introduction, and what measures would best stop a continued spreading of the evil when once introduced into a breeding place. The treatises are to be sent, under the usual formalities, to the office of the German Fisheries Union, 9, Leipziger Platz, Berlin. The competition for the prize is to be an international one, and the treatises may be written in German, English, or French. The final term is October 1, 1882.

WE have received specimens of the diaries published by Messrs. De La Rue. While their beauty and convenience commend them to everybody, they ought to be of special value to lovers of science, as they contain so many scientific data. Their get-up and general utility are beyond praise.

THE *Comptes rendus* of the Paris Academy of Sciences for December 20 is entirely occupied with the discourses pronounced at the funeral of M. Michel Chasles by representatives of the various bodies with which the deceased member was connected—MM. J. Bertrand, Bouquet, Laussedat, Dumas, and Rolland.

AT the last meeting of the St. Petersburg Society of Gardening M. Grigorieff made an interesting communication on Japanese gardens. The Japanese are most passionate lovers of gardening, which is carried on by all classes of society, from the great palaces to the most humble houses. Gardening, as well as the art of making bouquets, is taught in schools, and nowhere else in Europe are there so many gardens as in Japan. The species cultivated in the small private gardens are mostly miniature representatives of great trees. All new species and varieties of garden flowers and trees are sold at high prices and become known throughout the country with great rapidity. M. Grigorieff exhibited during his lecture a most interesting collection of photographs of Japanese gardens.

THE Russian scientific bodies continue to express their sympathy with Prof. Mendeleeff on the occasion of the refusal

by the Academy of Sciences to admit him as Member of that body. The Russian Chemical and Physical Society, while electing him Honorary Member, has presented him with an address in which it is stated that the Society considers him "to be a chemist who has no equal among Russian chemists." Many scientific bodies, as the University of Kieff, the Society of Hygiene, &c., have elected him Honorary Member or President. A public subscription has been opened for the institution of a prize bearing his name, and a great dinner was given in his honour by the St. Petersburg *savants*, among whom we notice the most eminent Russian Members of the Academy of Sciences. It is worthy of notice that Professors Korkin and Setchenoff, as well as the late M. Hilferding, the Panslavist explorer of Slavonian literature, met at the hands of the Academy of Sciences the same fate as M. Mendeleeff.

THE law for the isolation of the French National Library has been adopted by both Houses of the French Parliament, and the necessary expropriation for the great work will begin immediately.

AT a recent sitting the Municipal Council of Paris voted a sum of 400*l.* for the establishment of a School of Chemistry. It will be opened free to the pupils of the several Municipal schools who are desirous of practice in chemical industries.

THE French Government is to establish in Egypt a school of Egyptology, which will be directed by M. Maspero, now Professor of Egyptology to the College of France. This creation will be the third school established abroad at the expense of the French Budget. The two others are one at Rome and the other at Athens.

THE Thirteenth Annual Report of the Eastbourne Natural History Society testifies to the Society's continued prosperity. At the meeting of November 19 Dr. Royston Pigott read an interesting paper on "The Limits of Human Vision."

THE *Proceedings* of the Belfast Natural History and Philosophical Society for 1878-80 contains, besides several general papers, a few natural history papers of local interest, including one (with illustrations) on Irish Spiders, by Mr. Thomas Workman.

A BALNEOLOGICAL and a patent-protection exhibition will be held at Frankfort-on-the-Main in 1881.

## OUR ASTRONOMICAL COLUMN

VARIABLE STARS.—Amongst the stars which, from a comparison of the various catalogues, exhibit more or less strongly signs of variability, may be mentioned the following, which we take in order of right ascension; the positions are for the year 1880:—

1. Lalande 2037-8. R.A. 1h. 3m. 24s., N.P.D. 38° 30′·5. On September 29, 1790, Lalande rated this star 10m., and on December 27 following, 8·9; the B.D. (by which letters we refer to the Bonn *Durchmusterung*) has 7·0. Harding marks it a ninth.

2. 40 Cassiopeæ. R.A. 1h. 28m. 57s., N.P.D. Lalande calls this star 4m. in September, 1789, and 6 in 1790. In Argelander's zone No. 167 on January 1—, 1843, it was estimated 7, yet in the B.D. it is 5·2. In the first Radcliffe catalogue, where great attention was given to the magnitudes, it is 4·7; Piazzi has 6, Groombridge 5·6, and Heis the same.

3. Lalande 4864-5. R.A. 2h. 32m. 15s., N.P.D. 40° 57′·4. Estimated 9 in September, 1790, and 7½ in January following; but it has since been discerned with the naked eye, Heis calling it 6·7. The B.D. has 6·7. It is not in Houzeau.

4. Bradley 396. R.A. 2h. 53m. 13s., N.P.D. 8° 59′·8. Lalande rated this star 4·5 in November, 1789, and 7 in March, 1790. Groombridge, who made six observations for position, estimated it 7, Heis and Carrington 6, while it is 5·5 in the B.D.

5. 35 Camelopardi. We have already referred to the marked discordances in the estimates of the magnitude of this star in the various catalogues. Dembowski has directed attention to the probable variability of the principal component, and the star certainly deserves more regular attention at the hands of observers than it has yet received. R.A. 5h. 54m. 58s., N.P.D. 38° 25′·5. The estimated magnitudes vary from 5·5 to 8.

6. Attention may be once more directed to the star which Rümker compared with Encke's comet at Paramatta, N.S.W., on June 19, 1822, and which he rated at the time 4·5. Whether it really attains this degree of brightness is not yet certain; it is however 6·0 in the *Uranometria Argentina*, and was observed as low as 8m. in 1873. The B.D. says 6·5. Its light is a full yellow. R.A. 7h. 23m. 15s., N.P.D. 91° 39′·5.

7. 65 $b^2$ Geminorum. R.A. 7h. 22m. 21s., N.P.D. 61° 50′·3. Lalande rated this star as low as 8½ in March, 1794, but calls it 5½ in February following. Bessel estimated it 7; all other observers say 5 or 5·6.

8. 16 Leonis Minoris. R.A. 9h. 42m. 51s., N.P.D. 49° 48′·7. D'Agelet has 5 and 7·8, Lalande 6½, Piazzi 8, the first Radcliffe Catalogue 6·6, Bessel, Taylor, and the B.D. 7, Houzeau 5·6, but neither Argelander nor Heis included it amongst the stars visible to the naked eye.

9. Lalande 19034. R.A. 9h. 34m. 49s., N.P.D. 113° 2′·7. It appears strange that a star isolated as this is should not have been more frequently observed on the meridian, if always as bright as say 5m. D'Agelet and Piazzi have not got it; Lalande calls it 4½ on March 21, 1797; Argelander has 6 on March 6, 1850, 4 on February 16, 1851, and 5 on March 8, 1852; Heis and Houzeau call it 5, and Gould 5·2.

If we may rely upon the observations of Kirch early in the last century there would appear to be sensible changes in the relative brightness of $\beta$ and $\delta$ Scorpii; on January 17, 1704, he writes: "$\beta$ und $\delta$ erschienen fast im gleicher Grösse, jedoch $\delta$ ein wenig heller (jetzt ist $\beta$ 2, $\delta$ 3 grösse)," while on April 1 following he records "$\delta$ merklich grösser als $\beta$." Argelander and Heis estimate $\beta$ and $\delta$ respectively 2 and 2·3; Gould has no sensible difference.

10. Lalande 38405. R.A. 20h. 0m. 16s., N.P.D. 94° 45′·4. This star was rated 6 on July 15, 1794, 7 on August 15, 8 on August 20, and 7½ on August 30 of the following year. It is 8 in Bessel, 7 in Wolfer's map, one of the series of the Berlin Academy, and 6·7 in Heis and Houzeau. Gould does not give it. It might be inferred from Lalande's observations that the period is not very long.

11. 33 Capricorni. Chacornac says of this star: "Observée tantot plus brillante, tantot moins qu'une étoile de 7$^{me}$ grandeur dont elle est voisine:" the seventh magnitude alluded to being, it may be presumed, O.A. 21386, which follows 2m. 12s., 9′·4 to the south. 33 Capricorni is 5·6 in Argelander, 6·5 in Heis and Behrmann, and 5·7 in Gould; it is one of Chacornac's red stars, Gould also calls it red. The evidence of variability in this case seems to rest with Chacornac. R.A. 21h. 17m. 21s., N.P.D. 111° 21′·5.

12. 17 Andromedæ, a star previously noted in this column as variable. In the "British Catalogue" it is rated 4; Bradley and Piazzi call it 7; Lalande's three estimates are 5, 5 and 4; D'Agelet has 3·4 and 6; the first Radcliffe catalogue 3·9, and the B.D. 4·2; estimates from 4 to 7 are therefore sufficiently confirmed: the variation may be slow, but the star certainly deserves attention. R.A. 23h. 32m. 15s., N.P.D. 47° 24′·1.

THE COMET 1873 VII.—The comet discovered by Coggia at Marseilles on November 10 and by Winnecke on November 11 was soon lost in Europe from its rapid southerly motion; indeed the observations extend over less than a week. The elements exhibited a similarity to those which had been assigned to a comet detected by Pons in February, 1818, but very imperfectly observed, and Prof. Weiss, the present director of the Imperial Observatory at Vienna, was at the trouble of examining the question of possible identity as closely as the data permitted. He formed three normal positions from the small number of observations—for November 11, 13, and 15—and under the condition that the first and third normals should be exactly represented he ascertained how the second one was represented on the assumption (1) that the orbit was parabolic; (2) that the period of revolution corresponded to the interval between the perihelion-passages in 1818 and 1873, or 55·82 years; and (3) that the comet had completed eight revolutions in this interval, or that the period extends only to 6·977 years. As a matter of figures the agreement was found to be slightly closer for hypothesis (3) than for the other two, the parabola showing the largest differ-

ence. Hence so far as the paucity of data in 1873 enabled any judgment to be formed, the preference appeared to belong to the revolution in 6·977 years. On this supposition there would be a very near approach to the orbit of Jupiter near the ascending node, which would render possible an amount of perturbation at some past time, that might have fixed the comet in an orbit of such limited dimensions.

If the comet were really revolving in an elliptical orbit with this period of revolution, neglecting perturbations, which might however be sensible since 1873, it would be again due at perihelion about November 24 in the present year, in which case its track in the heavens would not be very different from that followed in 1873. We are not aware if any search has been made for the comet. It would have been possible to have decided in 1873 if a short period were admissible could observations have been obtained in the southern hemisphere: this was not done, and the identity of the comets of 1818 and 1873 remains therefore open to conjecture; but it must be borne in mind that the data in the former year are in a high degree uncertain.

PECHÜLE'S COMET.—Elements computed by the discoverer from observations at Copenhagen on December 16, 17, and 20, have a general resemblance to those of the comet of 1807, so elaborately discussed by Bessel, but after his resulting period of many centuries, and considering the position of the orbit in the system, there can be of course no question of identity of these bodies. At Greenwich noon on January 1 the comet's position will be, by Pechüle's elements, in R.A. 20h. 1·9m., N.P.D. 70° 24′, and on January 5 in R.A. 20h. 19·6m., N.P.D. 68° 30′.

## CHEMICAL NOTES

MM. HAUTEFEUILLE AND CHAPPUIS, in continuing their investigation of the conditions under which oxygen is transformed into ozone, have shown in *Compt. rend.* that the character of the electric discharge to which the oxygen is subjected largely influences the quantity of ozone produced. If the discharge assume the character of a luminous shower the maximum amount of ozone is produced, the temperature of such a discharge being lower than that of the ordinary *effluve*. The production of this special form of discharge is ensured by mixing with the oxygen a small quantity of a foreign gas whose physical properties are dissimilar from those of oxygen; of the gases experimented with silicon fluoride has given the best results. If nitrogen be the foreign gas the transformation into ozone is greater than when pure oxygen is employed, but the discharge is not altogether luminous. Hydrogen is more effective than nitrogen. The presence of carbon dioxide also insures a large amount of ozonation. In their earlier experiments on the liquefaction of ozone the authors only succeeded in obtaining a mist in the Cailletet tube when the pressure was suddenly withdrawn. They now find that if a mixture of carbon dioxide and oxygen which has been ozonised at a low temperature be submitted to the action of the silent discharge at − 23°, and be slightly compressed, the gas acquires a deep blue colour, and after a time a blue liquid is produced. At − 88° the liquid is very dark blue. When carbon dioxide is decomposed by the electric spark at − 23° a blue gas is produced, and at a certain pressure (exact pressure is not mentioned) the undecomposed carbon dioxide condenses to a blue coloured liquid. By this experiment the authors think they have proved that ozone is one of the products of the decomposition, by the spark, of carbon dioxide.

THE heat of formation of benzene has recently been measured by Thomsen (*Berliner Berichte*) and by Berthelot (*Compt. rend.*). The results show considerable differences: but from the accounts of the experiments, Thomsen's number seems the more trustworthy. For the heat of combustion of gaseous benzene Thomsen finds the number 805,800 heat units; Berthelot the number 776,000. For the heat of formation of gaseous benzene, at constant volume, from amorphous carbon and hydrogen, Thomsen finds − 20,120 heat units; Berthelot, on the other hand, finds + 5800. Berthelot does not state whether this number is calculated for constant volume or constant pressure. Thomsen makes certain theoretical deductions from the value which he has found for the heat of formation of benzene, basing these on his calculations for the heat of formation of "singly- and doubly-linked" carbon atoms (see NATURE, vol. xxii. p. 608): he concludes that the generally accepted hexagon formula for benzene is probably incorrect, and that a formula in which each carbon atom is "singly linked" to three others is to be preferred.

BERTHELOT finds the heat of formation of gaseous dipropargyl—$C_6H_6$—a metamer of benzene—from amorphous carbon to be − 64,800 heat units. The instability and easy polymerisation of this body are explained by the great absorption of heat which occurs in its formation. Attempts to transform dipropargyl into benzene were unsuccesaful. Berthelot has also determined the heats of formation of various hydrocarbons, and finds certain constant differences between the successive members of homologous series.

HERR V. MEYER describes in the *Berliner Berichte* a very elegant modification of his method of determining vapour densities, whereby the specific gravities of permanent gases may be readily measured at very high temperatures. At the highest temperature of a Schlösing's furnace (about 1400°) the density of hydrogen vapour was normal. It has now been shown that the coefficients of expansion of the following gases are not changed at very high temperatures: tellurium, sulphur, nitrogen, oxygen, hydrogen, mercury, carbon dioxide, hydrochloric acid, arsenious oxide.

THE following numbers for the vapour densities of tellurium and selenium have been recently obtained by Deville and Troost (*Compt. rend.*):—Selenium at 1420° = 5·68 (calculated = 5·54), tellurium at 1440° = 9·0, at 1390° = 9·08 (calculated = 8·93).

IN the *Wien. Acad. Berichte* Herr Offer describes the results of experiments which he thinks show that Guthrie's cryohydrates are merely mixtures of various salts and ice: alcohol dissolves the ice, leaving a "skeleton" of undissolved salt; cold water dissolves the salt, and leaves the ice with the form of the cryohydrate. Solution of a cryohydrate is attended with the same thermal change as solution of the salt and ice separately.

THE connection which exists between the opium and cinchona alkaloids, and between both of these groups of compounds and pyridine, has been recently made more apparent by the work of Herr E. v. Gerichten (*Berliner Berichte*), who has shown that the so-called apophyllenic acid—obtained by oxidising the opium alkaloid cotarnine—is really the acid methyl salt of pyridine dicarboxylic acid, which acid is obtained from the cinchona alkaloid cinchonine, and which when heated with lime yields pyridine.

WHEN an aqueous solution of potassium, sodium, or lithium chloride, or potassium nitrate is kept for some time in a vertical tube, the upper part of which is maintained at a considerably higher temperature than the lower part, diffusion of the salt from the hotter to the colder part occurs, according to M. C. Soret (*Naturforscher*). The amount of diffusion in a given time depends upon the original concentration of the solution, and is also connected with the molecular weight of the salt used.

M. DUFET stated some little time since that the refractive index of a mixture of isomorphous salts in solution is equal to the mean of the indices of the components. Herr Fock (in *Zeitschrift für Crystallographie*) concludes, from measurements of the refractive indices of solutions of thallium and potassium alums, of lead and strontium thiosulphate, and of magnesium chromate and sulphate, that Dufet's statement does not hold good in all cases: for the second pair of salts mentioned above it is approximately correct. In *Compt. rend.* Dufet gives numbers showing that his statement applies to a mixture of magnesium and zinc sulphates.

IN *Compt. rend.* M. Demarcay describes two new compounds containing sulphur, nitrogen, and chlorine, viz., SNCl and $(SN)_3Cl$: the former prepared by passing chlorine into a solution of nitrogen sulphide in chloroform, and the latter by adding nitrogen sulphide to a solution, in chloroform, of the compound SNCl. SNCl is partly decomposed by heat, in accordance with the equation $2SNCl = N_2 + S_2Cl_2$.

THE relation between the total energy developed in the chemical reactions which occur in various kinds of galvanic batteries and the energy which appears in the form of current electricity, has been recently studied by Thomsen (*Wiedemann's Annalen*) using a thermal method of measuring the total energy. He finds that the whole of the energy developed in the chemical change appears as electric energy in Daniell's battery (with closed circuit), and in those forms of batteries in which the metallic surface of the negative electrode is not changed by the electrolytic process; when nitric acid is used as electrolyte the same total conversion of one into another form of energy is

nearly realised, but the gradual absorption, by the liquid, of reduction products, tends to cause a deviation from this result.

BERTHELOT has recently studied (*Compt. rend.*) the action of air and of hydrochloric acid in presence of air, on pure mercury. He confirms the generally accepted fact that pure mercury is very slowly and superficially oxidised by the action of air at ordinary temperatures. If gaseous hydrochloric acid is shaken with mercury in presence of air mercurous chloride and water are produced. This reaction ($Hg_2+2HCl+O=Hg_2Cl_2+H_2O$) is attended with the evolution of 53,400 thermal units, whereas the oxidation of mercury ($Hg + O = HgO$) is attended with the evolution of only 21,100 units. The action of hydrochloric acid in presence of air on copper ($Cu_2 + 2HCl + O = Cu_2Cl_2 + H_2O$) is accompanied by the evolution of 26,500 thermal units, hydrochloric acid in absence of air is, as is well known, almost without action on metallic copper.

IN the course of his investigation into the action of phosphorus on hydriodic acid, Damoiseau (*Compt. rend.*) describes a method for preparing phosphonium iodide in a state sufficiently pure for general use. Ten parts ordinary phosphorus in small pieces are allowed to react for some time on twenty-two parts of an aqueous solution of hydriodic acid (saturated in the cold); two parts iodine are added, and the phosphorous acid which is produced is separated from the crystals of phosphonium iodide by washing with aqueous hydriodic acid.

A SERIES of compounds, derived from monohydric alcohols, in which the "hydroxylic hydrogen" of the alcohol is replaced by aluminium, is described (*Chem. News*) by Gladstone and Tribe. These bodies are prepared by the action of aluminium in presence of aluminium iodide on the alcohol. The new substances are solids, melting to clear liquids which do not solidify at temperatures much below the melting points of the solids; they are decomposed by water with formation of aluminium hydrate and the corresponding alcohol.

THE sulphides of vanadium have been investigated by Kay (*Chem. Soc. Journ.*). The compounds obtained by Berzelius by the action of sulphuretted hydrogen on solutions of vanadium salts are shown to contain oxygen in addition to vanadium and sulphur, but no definite formula can be assigned to any of these bodies. Vanadium trisulphide $V_2S_3$ is obtained by the action of dry sulphuretted hydrogen on heated vanadium trioxide, as described by Berzelius. When this compound is heated to bright redness in hydrogen, it is reduced to the disulphide $V_2S_2$, and when heated with sulphur to 400° it is converted into the pentasulphide $V_2S_5$.

IN the *Chem. Soc. Journ.* Kingzett describes experiments on the atmospheric oxidation of phosphorus which seem to prove that ozone and hydrogen peroxide are simultaneously produced when air is drawn over phosphorus partially immersed in water.

MANY so-called basic sulphates of iron have been from time to time described: of the fifteen which are generally recognised as probably existing it would appear from Pickering's experiments (*Chem. Soc. Journ.*) that only one, viz. $2Fe_2O_3 \cdot SO_3$, actually exists.

DR. SYDNEY MARSDEN has recently experimented on the action of boron on various metals at high temperatures. He finds (*Proc.* R. S. Edin. and *Chem. Soc. Journ.*) that silver dissolves amorphous boron, and that on cooling, pure boron is obtained partly in the graphitoidal, partly in the adamantine form. Copper combines with boron to form the compound $B_2Cu_3$.

PROF. BELLATI has published in pamphlet form, under the title "Proprietà termiche notevoli di alcuni Ioduri doppi," an extended and careful series of observations of the specific gravities, specific heats, thermal expansions, and thermal changes which accompany changes of colour and structure, of several double iodides of mercury, more especially of the three salts $HgI_2 \cdot 2AgI$, $HgI_2 \cdot 3AgI$, and $HgI_2 \cdot Cu_2I_2$.

HERR HAASS describes in the *Berliner Berichte* a simple method of illustrating the existence of the so-called "critical pressure" described in this journal by Carnelley. A small piece of mercuric chloride is placed in a glass tube which is closed at one end, and communicates at the other with a Bunsen pump. So long as the manometer registers less than about 400 mm. pressure it is not possible to melt the mercuric chloride by heating it; the salt passes at once from the solid to the gaseous state. But immediately the pressure rises above about 420 mm. the mercuric chloride melts.

IN studying the condensation products of aldehyde Prof. Lieben has obtained (*Wied. Akad. Ber.*) a new alcohol belonging to the same series as glycerin, viz. $C_4H_7(OH)_3$. The new compound, called butenyl glycerin by Lieben, is a syrupy, sweet-tasting liquid, soluble in water, boiling at 172°–175° under a pressure of 27 mm. It forms a triacetin analogous in properties to the natural fats; when heated with oxalic acid its behaviour is similar to that of glycerin: formic acid is produced along with an oily, strongly-smelling substance which has not as yet been fully examined.

## PHYSICAL NOTES

MONS. A. ANGOT proposes a new formula for calculating altitudes from barometric observations, based upon that given originally by Laplace. The existing method of calculation from observed monthly or annual means is found, as Plantamour has shown, to be defective, since its results exhibit an uncertainty that varies with the season, an elevated station appearing to be higher by day and in summer than at night or in winter. As an example, when the height of the Great St. Bernard is measured by comparison of barometer observations between that place and Geneva, it would appear that the height of the Great St. Bernard exhibits a diurnal variation of 17 metres in winter and of more than 47 metres in summer; while the mean of the June observations gives a height of 25 metres higher than that found from the January numbers! These anomalies M. Angot explains by the facts that the mean temperature between the stations is not exactly equal to the half sum of the two temperatures, and that the weight of the air between the two stations is on the other hand greater when the mean temperature is low. The rather complicated formula proposed by M. Angot gives the difference in altitude by calculating directly the height of each station above an imaginary plane at which the barometric pressure is equal to 760 millims. No empirical coefficients are needed in this case, the standard constants of Regnault and others for air and aqueous vapour being taken. M. Angot has recalculated from his formula a new set of tables, involving all the corrections that must be applied to the older tables of the Bureau des Longitudes.

IN a recent number of the *Journal de Genève* M. Colladon has pointed out that a poplar or other tall tree may, if its roots strike into damp soil, serve as a lightning-conductor to protect a house; and he thinks he has verified this conjecture by examination of a number of individual cases of lightning-stroke. In the case however where the house stands between the tree and a piece of water, a pond or a stream, the shortest path for the lightning from the tree to the wet conductor may be through the house!

VYON ZOCH has described a new kind of electric dust-figures, which he regards as having an important bearing upon the theory of discharges in vacua, being in opposition to the views of Crookes. Tubes of 1 to 3 centims. diameter, and from 10 to 30 centims. in length, were closed at both ends by corks pierced to receive copper wires. In the tubes were placed various powders, bronze powder being chiefly used in preference to others, which being lighter adhered to the sides of the tube. One wire was then connected with the positive conductor of an electric machine; from the other the repelled electricity dissipated itself into the air. In other cases the discharges of Leyden jars were employed. The experiments were all conducted at atmospheric pressure. When thus treated the bronze powder arranged itself in beautifully-marked ridges or strata, varying in regularity according to the original distribution of the powder. A space free from all traces of powder was observed to surround the positive pole. Uusually there was a corresponding accumulation about the negative pole. These ridges or striations may be compared to the stratifications observable in Geissler-tubes; and Herr Zoch shows that variations in the strength of the electric discharges, in the width of the tubes, &c., produce upon these figures similar effects to those they produce on the luminous striæ of vacuous tubes. In this present case a mechanical repulsion of the particles lying near the poles undoubtedly takes place; and the author of this research believes that the presence of light at the poles of the Geissler tube may be similarly accounted for on the hypothesis that the luminous regions are those of less density than the non-luminous. Since the bronze powder is heaped up mostly about the negative pole the inference is that at the negative pole of a Geissler tube the residual gas has a greater density than at

any other part. The stratifications produced by electric discharges through flames may be similarly explained; and these researches have an obvious bearing on the structure of Lichtenberg's well-known figures.

MONS. MERCADIER has been devoting some attention to the subject of the photophone, and more particularly to the production of sounds by the simpler forms of the instrument, in which a selenium receiver with its electrical connections is dispensed with. The musical photophone—or, as M. Mercadier chooses to style it, the radiophone—may be described as a sort of optical siren, in which a rotating disk pierced with holes is interposed in the path of a period of rays of light, causing intermittences of regular period varying with the speed of the disk. Our readers will remember that such a beam falling on a simple disk of metal or of hard rubber throws it into vibration, and it emits a note corresponding in pitch with the frequency of the intermittences of the light. In Prof. Bell's actual instrument this "siren" was a heavy disk of brass pierced with holes. M. Mercadier prefers a disk of black paper gummed upon a glass disk in order to get rid of the whistling sounds which even a gentle current of air produces on the brass disk. It may be noted in passing that M. Duboscq has independently constructed similar disks. The receiving disks were fixed in a suitable holder at the end of a short india-rubber hearing-tube. M. Mercadier finds that when opaque disks of zinc, copper, and other substances are employed to receive the beams, very little difference in the loudness of the sounds can be perceived, whether the disks are polished or not. But the thickness of the disks is of great importance, thin ones answering much better than those a little thicker. With transparent laminæ such as glass and quartz, M. Mercadier obtains strong effects, whereas Prof. Bell found only feeble results with these substances. The degree of polish is here unimportant also; but a film of smoke or white paint, or of metallic silver on the front of the disk, diminishes its powers, while, on the contrary, the loudness is augmented by blackening the back of the disk. M. Mercadier employed as sources of light the lime-light and flames of petroleum fed with oxygen.

HERR F. KLOCKE has lately discovered an anomalous property in hyposulphite of lead in respect of its action on polarised light. This substance usually exhibits circularly polarised light: but Klocke has found that plates cut perpendicularly to the optic axis, when viewed in the field of a polariscope by parallel rays of light, appear unequally bright, being divided by dark bands into six sectors, of which opposite pairs are equally bright. In convergent light, moreover, the ordinary ring-figure of a uniaxial crystal is not seen, but instead there appears in each sector a figure of the form characteristic of the ordinary biaxial crystal, and having the plane of the optic axes perpendicular to the neighbouring edge of the crystal. The explanation of this curious phenomenon appears to be that there is some anomaly in the molecular structure of the crystals, by virtue of which the six portions are compressed equally each in direction perpendicular to the neighbouring face of the prism.

IN the Vienna *Berichte* for June, 1880, Victor von Lang describes a form of dichroiscope, in which a small improvement upon the common form has been made. Usually the small square aperture through which light is admitted to the rhomb of spar is fixed rigidly to the tubular holder of the latter. In the new form the square aperture is cut in a diaphragm fastened to an outer tube, which can be rotated round the inner. The advantages gained in permitting the rhomb of spar to be turned independently of the aperture are obvious. A plano-convex lens of small magnifying power is added as usual as an eyepiece at the other end of the rhomb.

M. AMAGAT has experimented on the compressibility of oxygen gas in an apparatus in which the working fluid for transmitting the pressure was mercury. Since the experiments of Regnault it has been commonly assumed that the absorption of the gas by mercury at high pressures and temperatures rendered inexact any such experiments. M. Amagat however finds that the absorption is almost insensible, an oxygen manometer and a nitrogen manometer giving identical indications for several days, even with temperatures varying up to 100°.

MM. HAUTEFEUILLE AND CHAPPUIS have continued their researches on the liquefaction of ozone, which they have lately liquefied in the presence of carbonic acid. They believe the point of liquefaction of ozone to be very near that of carbonic acid; and on mixing ozonised oxygen with carbonic acid and submitting it in a capillary tube to a slow pressure at a temperature of − 23° (obtained by the evaporation of methylic chloride), they obtained a liquid separated by a distinct meniscus from the gas. This liquid was of a clear blue tint, as was the compressed gas above it. If the substance is then allowed to expand gently and immediately compressed, the liquid becomes much more blue, owing to the greater proportion of liquefied ozone. The blue tint thus characteristic of ozone under pressure proves it to be present in the gases which result when the silent electric discharge is passed through carbonic acid gas for some hours.

EDISON has lately patented a "*webermeter*." This is an instrument for measuring the amount of electric current flowing through a circuit, or in other words a *meter* for electric currents to tell the number of webers that have been supplied. The name is at least in accordance with the inventor's usual abundant ingeniosity.

IN the *Comptes rendus* M. Gouy publishes an extract from a memoir presented by him to the Académie des Sciences, on the propagation of light. In this memoir he proposes to examine the particular case of propagation of luminiferous waves, in which, while the direction of the propagation of the movement is constant, the intensity of the waves or of the source of light varies. This problem, which has doubtless been suggested to the author by considerations derived from the photophone, affects the whole question of the measurement of the velocity of light, whether by the methods of occultations of Rœmer and Fizeau, or by that of aberrations (in the rotating mirror), as devised by Foucault. The former case only is treated of in M. Gouy's paper. Setting aside at first the case of dispersive media, and restricting the question to isotropic media, M. Gouy investigates mathematically whether the velocity of propagation of the amplitude is the same as that of the wave, and finds that this is the case only for those waves for which the differential equations contain no terms beyond those of the second order—those in which the vibration has virtually attained to the steady condition. For such waves moreover in dispersive media the amplitude is not propagated with the same velocity as the waves themselves, but the amplitude itself varies according to a complex function of the wave-length according to an ascertainable periodic law. If we remember rightly, a similar hydrodynamic investigation of the rate of propagation of waves in water was made some years ago by Prof. Osborne Reynolds, with the result that the effective wave-front only travelled at half the velocity of the steady waves. The inference is that that which physicists usually term "the velocity of light" is only the rate of propagation of the wave-front, which is slower than the true velocity, the retardation being greatest for the vibrations of greatest wave-length.

ANOTHER new property of selenium is claimed as the discovery of M. Blondlot. He states that when selenium is rubbed upon platinum, each metal being connected with a terminal of a capillary electrometer, a current is observed. This current, which is observed to pass through the electrometer from the platinum to the selenium, appears therefore to differ from the tribo-electric currents discovered by Becquerel, and which were always in the same direction as the thermo-electric currents which would have been produced had the surfaces of friction been directly heated. The true thermo-electric current of a selenium-platinum pair is, according to M. Blondlot, from selenium to platinum through the heated junction. One curious point stated by M. Blondlot is that no indication whatever is obtained upon the capillary electrometer by friction between two metals, or between two insulators, or between a metal and an insulator. The electrometer in the selenium experiment indicated a difference of potential about equal to that of one Daniell's cell.

MAGNUS AND TYNDALL found carbonic acid to have a considerable absorbent action on radiant heat. Dr. Lecher (*Wien. Acad. Anz.*) has lately made new observations, especially as to absorption of solar radiation by the carbonic acid in the atmosphere. Experiments with a gas-lamp and glass cylinder first showed that carbonic acid in a length of 214 mm. gave passage to 94·8 per cent. of the radiation; 536 mm. 93·8 per cent.; 917 mm. 89·0 per cent. At Greifenstein, outside of Vienna (chosen for pure air), the sun's rays also were proved to undergo considerable weakening in passage through carbonic acid gas. A layer of this gas one metre thick absorbed about 13 per cent. when the sun had an altitude of 59°; the number however diminished in proportion as the sun got lower. This shows that the absorption of solar radiation by carbonic acid is selective, and that the

absorbable wave-lengths become more rare the greater the atmospheric layer the rays have already traversed. The author calculates from his experiments the proportion of carbonic acid in the atmosphere, finding it 3·27 in 10,000 parts by volume; a number agreeing so well with results of chemical analysis as to indicate that this is a good way of determining the carbonic acid in the atmosphere and its variations, applicable, too, at heights where direct measurements are impossible.

HERR WINKELMANN proves by experiment (*Wied. Ann.* No. 11) that the heat-conduction of ethylene decreases somewhat with increased pressure. The pressure was varied from 10 to 740 mm. (Comparative experiments with air showed no influence of pressure.) The author explains the phenomenon by the divergence of ethylene from Boyle's law. The action of cohesion-forces between the molecules is indicated by that fact; and this will cause, at each collision, a temporary retardation of the straight movements, which effect will occur oftener the greater the number of collisions (*i.e.* the greater the density). Hence this retardation will increasingly affect the velocity with which two contiguous layers of different temperature exchange the energy of their motions.

A REMARKABLE fall of rain in Austria and neighbouring parts on August 11-15 this year, has been closely investigated by Dr. Hann (*Wien. Akad. Anz.*), on the basis of data from 260 stations in Austria-Hungary, Bavaria, Switzerland, and Saxony. This fall caused the Danube at Vienna to reach (on the 18th) its highest summer level in this century. The rain began in Siebenburgen and south-east Hungary on the 11th, and in general went from east to west. It was most extensive on the 12th, and the heaviest fall was in Salzkammergut and neighbourhood. The rain-area is found to lie on the west and north-west side of the area of lowest air-pressure, and to stretch westwards far over the border of the minimum region. Near the centre of lowest pressure the precipitation was much less than in several parts distant from it. The non-existence of a minimum-producing power of rainfall (contrary to common views), and the incapability of so great rainfall as that in the present case attracting a minimum and influencing its propagation, are noteworthy. The general conclusion arrived at is that no relation is demonstrable between barometric variation and rainfall; the fall of the barometer does not primarily depend on the rainfall, and is not perceptibly influenced by it. Dr. Hann finds this confirmed by an examination of several other heavy rainfalls in their relation to distribution of air-pressure.

THE salt and the ice in cryohydrates have been regarded by Prof. Guthrie as in chemical combination. In 1877 Herr Pfaundler expressed the view that cryohydrates were merely mixtures of salt and ice. This view is also maintained by Herr Offer, who in a recent paper to the Vienna Academy raises various objections to the existence of cryohydrates as chemical compounds. The numbers expressing the quantities in which the water unites with the salts in various cryohydrates, indicate no stoichiometric law, and tell much rather in favour of chemical mixtures. No cryohydrate forms a clear and pure crystal, but always an opaque confused crystalline mass. The phenomena which occur when cryohydrates are brought into alcohol and into water are considered to be against Prof. Guthrie's view. The heat-absorption of cryohydrates in dissolving, as compared with that of the salt and ice separately, only presents differences which lie within the errors of observation. Further, Herr Offer compared the specific gravity of several cryohydrates with those of their constituents, and found pretty close agreement.

FROM recent magnetic researches Herr Auerbach (*Wied. Ann.* No. 11) finds the temporary magnetism of cylindrical bodies, *ceteris paribus*, proportional to the mass; greater the greater the length; the less the thickness; the greater the density; dependent only on form, not on size; in the case of nickel, according to density and force, a quarter to half as much as in iron. It increases with magnetising force, first proportionally, then (except with very small density) more quickly, and at last more slowly. The quick increase is greater the denser the body. The turning point is, for the same density, at the same place, but with stronger forces the greater the density; for magnetic saturation of powders extremely strong forces are necessary. Herr Auerbach theorises on these results.

ANOTHER paper on magnetism in these *Annalen* is by Herr Baur, and deals with the "function of magnetisation" for very small magnetising forces; the influence of temperature on it; the magnetisability of iron at very high temperatures; Gore's phenomenon; and the function for varieties of iron. Among other results, the smaller the magnetising force the greater is the influence of temperature on the function in question. Up to a certain force the function increases with increase in temperature, but beyond that it decreases. With weak forces the temporary magnetic moment rises quickly (with rise of temperature) to a maximum at red glow, then sinks quickly to *nil*; with strong forces it gradually sinks, with rise of temperature, to a very low value at red glow. With increased magnetic force Gore's phenomenon becomes more intense and prolonged, and it occurs at a higher glow. In ordinary iron the function of magnetisation reaches its maximum very quickly, in iron filings later, and in electrolytic iron very late.

## GEOGRAPHICAL NOTES

THE glacier of the Byeloukher Mountain, the chief summit of the Siberian or Great Altay, which has not been visited by men of science during the last fifty years, was recently explored by an expedition engaged in the study of the life of the West Siberian natives. After having crossed the 9000 feet high Alps of the Tchouya, the explorers descended into the pretty and wealthy broad valley of the Tchouya, whence, following the Arkhyt River, they soon reached the foot of the mighty Berel glacier. The glacier, which forms in its lower parts a *mer de glace* two miles long and 2800 feet wide, was accurately explored and surveyed during a week by the expedition from its lower end to a great ice-fall, where the travellers were compelled to stop their work before a moving wall of ice, while mighty masses of snow fell, one after the other, on the glacier from the neighbouring mountains. After having surveyed the glacier and made several drawings of the severe scenery which it affords, the travellers returned to the valley of Ouimon, and thence to the civilised towns.

THE astronomical determinations of positions which were made by M. Pyevtsoff during his journey from Khobdo through Mongolia to Kalgan, and from Ourga to Kosh-agatch, are published by Col. Scharnhorst in the last number of the *Izvestia* of the Russian Geographical Society. They are most welcome, as they come from a country where exact determinations are very scanty. —The same fascicule of the *Izvestia* contains M. Larionoff's catalogue of seventy-five determinations of heights in the northern and eastern parts of the province of Kouldja and in the mountains which border it north and east; and M. Severtsoff's map of his route on the Pamir Rang-koul, south-east of the Lake Kara-koul.

THE Russian travellers who have been engaged in the exploration of Central Asia are now returning to St. Petersburg. Col. Prshevalsky is expected every day, and the Russian Geographical Society, at its last meeting (December 15), elected the indefatigable traveller an Honorary Member. M. Potanin is already at St. Petersburg, and will soon give a lecture on his journey to Western Mongolia, as also M. Pyevtsoff, who travelled with merchants from Biysk to Khou-khou-khoto, and who during his journey collected much material for the correction of the map of Mongolia. M. Mushketoff, who has explored the glacier of Zarafshan (NATURE, vol. xxiii. p. 44), gave a lecture at the last meeting of the Russian Geographical Society on his excursion. This traveller, contrary to M. Severtsoff's experience, did not find in the Thian-Shan any traces of the glacial period.

THE *Kouban News* announces the appearance, in the Sea of Azoff, of a new little island, some 150 feet in diameter, and 10 feet above the level of the water. Its appearance was accompanied with a kind of marine eruption. It is 150 brasses distant from the shore, where a crevice has appeared.

THE organisation of the Polar meteorological station on the Lena is being actively carried out by Prof. Lentz. The director of the station will be M. Yurgens.

PROF. NORDENSKJÖLD is again thinking of fresh enterprises. At present a ship is being built at the Lena estuary, in which he intends to start on a new Arctic expedition in the summer of 1882.

PRINCE BORGHESE, the Italian African traveller, has arrived near Tripolis from Wadai. This is the first time that a traveller from Darfur has reached the Mediterranean by way of Wadai and Bornu.

THE Leipzig publishing firm of Ferd. Hirt and Son announce that Major Serpa Pinto's great African work of travel will be published in January, 1881.

## UNIVERSITY AND EDUCATIONAL INTELLIGENCE

ALFRED C. HADDON, B.A., Scholar of Christ's College, Demonstrator of Comparative Anatomy, and Curator of Zoology in the University of Cambridge, has been appointed Professor of Zoology at the Royal College of Science, Dublin.

THE movement to found a college at Dundee has been revived, and at a meeting last week it was announced by Dr. Baxter, the Procurator-Fiscal, that he was in a position to place 125,000*l.* at the head of a subscription for the purpose. Owens College, Manchester, is proposed as the model of the Dundee Institution.

DURING recent years much has been done in Russia by private initiative for primary education in natural science. Now we notice the creation at St. Petersburg of a special institution, the aim of which is to devise and collect apparatus and drawings for the teaching of natural science in primary schools. A special collection of objects intended for the illustration of science will be sent from school to school by the Committee, and lectures will be given in each school on the subject.

THE building of the new Siberian University is being briskly carried on. It will contain twenty large rooms for lectures, as well as spacious halls for the museum and library. The building for anatomy, as well as the hospital for clinical medicine, will be erected in accordance with the latest hygienic principles. A special building will be appropriated for the physical cabinet and the astronomical observatory.

THE Moscow University is closed for an indeterminate time because of the disturbances among medical students, and three hundred students are incarcerated in the town prison.

## SCIENTIFIC SERIALS

*Journal of the Franklin Institute*, December.—Boiler experiments, by Mr. Isherwood.—New electric motor, by Mr. Griscom.—The Sawyer electric light.—Proceedings of Institute, &c.

*Bulletin de l'Academie Royale des Sciences de Belgique*, Nos. 9 and 10.—Influence of liquids on the sound of sonorous bells which contain them or which are immersed in them, by M. Montigny.—On the chemical composition of the epidote of Quenast, by M. Renard.—On Caels and De Bennie, by M. Mailly.

No. 11.—On the compensation of a chain of geodetic triangles, by M. Adan.—Excretory apparatus of Trematodes and Cestode (3rd paper), by M. Fraipont.

*Rivista Scientifico-Industriale*, No. 21, November 15.—On spherohedry in crystallisation, by Prof. Bombicci.—On beats, the third sound of Tartini, &c. (concluded), by Dr. Crotti.

No. 22, November 30.—On some singular phenomena of geometrical optics, by Prof. Cassani.

*Reale Istituto Lombardo di Scienze e Lettere. Rendiconti*, vol. xiii. fasc. xvii., November 11.—On *Peronospora viticola* and the cryptogamic laboratory, by Prof. Garovaglio.—On measurement of the thermo-luminous radiations of the sun, by Dr. Chistoni.—Fourth series of researches and studies on the pelagic fauna of the Italian lakes (short *résumé*), by Prof. Pavesi.—The leprosy of Ancient Italy, especially of Comacchio, by Prof. Sangalli.

*Kosmos*, September 1880, contains:—Theodor Vuy, on the rehabilitation of shattered authorities; considerations on the education of the future.—Dr. Ernst Krause, sketch of the development history of the history of development, No. 3.—Dr. H. Müller, on the variability of alpine flowers.—Leopold Einstein, apprehension and comprehension, a study in the philosophy of language.—Short notices and extracts from journals. Literary and critical notices.

October.—Prof. Fritz Schultze, the transformation of human fundamental conceptions on the threshold of modern times.—Prof. Dr. Hörnes, on phacops and dalmanites, genera of trilobites and their probable genetic connection.—George Potonié, on the purport of the stony particles to be found in the flesh of the pear and generally in the Pomaceæ.—Dr. Fritz Müller, *Paltostoma torrentium*, a gnat with two forms of females, one with a mouth for honey-sucking the other with a mouth for blood-sucking (with illustration).—Short notices and extracts from journals. Literary and critical notices.

## SOCIETIES AND ACADEMIES

### LONDON

**Chemical Society**, December 16.—Prof. H. E. Roscoe, president, in the chair.—The following communications, &c., were made:—On the estimation of nitrogen by combustion, including the nitro-compounds, by J. Ruffle. The author recommends the use of the following mixture instead of soda-lime in the process of Will and Varrentrapp:—Two molecules of sodium hydrate, one molecule of pure lime, and one molecule of sodium hyposulphite; the substance before burning being mixed with about its own weight of a mixture of sulphur and wood charcoal. By this process good results were obtained with sodium nitrate, picric acid, &c.—Dr. Carnelly then showed some experiments as to the effect of pressure in raising the melting-points of ice, camphor, and mercuric chloride. By suspending a cylinder of ice (formed round the bulb of a thermometer) in a Torricellian vacuum and condensing the aqueous vapour by a freezing mixture, so as to keep the vacuum perfect, the author has raised ice to 180° C. before it melted. In the experiment shown, through an accident, the temperature only rose to 30° C. before the cylinder fell off the thermometer. Camphor which was boiling in a tube solidified when the pressure was diminished, though the heating was continued. Mercuric chloride, which under diminished pressure had been raised considerably above its melting-point, melted and boiled as soon as it was exposed to atmospheric pressure.—On some naphthalene derivatives, by Dr. Armstrong and Mr. Graham.

**Geological Society**, December 15.—Robert Etheridge, F.R.S., president, in the chair.—William Elijah Benton, Rev. George Clements, J. Kerr Gulland, Francis T. S. Houghton, George Bingley Luke, and William Mansell MacCulloch, M.D., were elected Fellows; and Prof. Luigi Bellardi of Turin, and Dr. M. Neumayr of Vienna, Foreign Correspondents of the Society. The following communications were read:—On the constitution and history of grits and sandstones, by John Arthur Phillips, F.G.S. In the first part of this paper the author described the microscopic and chemical structure of a large series of grits, sandstones, and in some cases quartzites, of various geological ages, noticing finally several sands of more or less recent date. The cementing material in the harder varieties is commonly, to a large extent, siliceous. The grains vary considerably in form and in the nature of their inclosures, cavities of various kinds and minute crystals of schorl or rutile not being rare. The author drew attention to the evidence of the deposition of secondary quartz upon the original grains, so as to continue its crystal structure, which sometimes exhibits externally a crystal form. This is frequently observable in sandstone of Carboniferous, Permian, and Triassic age. Felspar grains are not unfrequently present, with scales of mica and minute chlorite and epidote. Chemical analyses of some varieties were also given. The author then considered the effect of flowing water upon transported particles of sand or gravel. It results from his investigations that fragments of quartz or schorl less than one-fiftieth of an inch in diameter retain their angularity for a very long period indeed, remaining, under ordinary circumstances, unrounded; but they are much more rapidly rounded by the action of wind. It is thus probable that rounded grains of this kind in some of the older rocks, as, for example, certain of the Triassic sandstones, may be the result of Æolian action.—The chair was then taken by J. W. Hulke, F.R.S., V.P.G.S.—On a new species of *Trigonia* from the Purbeck beds of the Vale of Wardour, by R. Etheridge, F.R.S., president; with a note on the stratigraphical position of the fossil by the Rev. W. R. Andrews. In this paper the author described a species of *Trigonia* discovered by the Rev. W. R. Andrews in the "cinder-bed" of the Middle Purbeck series in the Vale of Wardour. The specimens were found in the railway-cutting one mile west of Dinton Station. The shell was referred to d'Orbigny's section "Glabræ" of the genus *Trigonia*, and named *Trigonia densinoda*. In its ornamentation it closely resembles *T. tenuitexta*, Lyc., of the Portland oolite, but is more depressed and lengthened posteriorly, and destitute of the antecarinal space which occurs in all known Jurassic "Glabræ." The escutcheon is remarkably large, and possesses transverse rugæ, as in the Neocomian "Quadratæ." The author regarded the species as a transition form connecting the two groups of *Trigoniæ* abovementioned. The description of the new species was accompanied by a note on the Purbeck strata of the Vale of Wardour by the Rev. W. R. Andrews.

**Meteorological Society**, December 15.—Mr. G. J. Symons, F.R.S., president, in the chair.—J. Coventry, J. W. Moore,

M.D., W. T. Paulin, J. Porter, and Capt. W. C. Smith were elected Fellows.—The following papers were read:—Report on the phenological observations for the year 1880, by the Rev. T. A. Preston, M.A., F.M.S. Agriculturally speaking the year may be considered as disappointing. Till June the weather was such as has rarely been experienced for farm operations. The severe cold of the winter broke up and mellowed the soil, and the dry open weather enabled farmers to clean their land from the excessive growth of weeds caused by the damp of the year before. The dry May was not favourable for the hay, which suffered severely in some places, but still a crop with far more real nourishment in it than would be obtained from a rank growth would have been secured had it not been for the terrible floods of July in the Midland Counties, which not only seriously injured the crop, so that it was frequently not worth the trouble of removing off the land, but also carried it entirely away in low-lying districts. The corn again, which was looking most promising till July, suffered much during that damp period, and had it not been for the subsequent fine weather would have been ruined. But the unfavourable season of 1879 produced very serious effects on vegetation, especially on trees and shrubs and their produce. The young wood of the trees was not ripened, and as a natural consequence the severe winter killed an enormous quantity of some kinds, and greatly injured others. "Laurustinus" was generally killed to the ground, and in some districts the destruction of other shrubs was severely felt. The evergreens in many cases lost large quantities of their leaves. Hollies especially are mentioned by several observers, and privet-hedges were sometimes quite leafless. With respect to fruit-trees, apples and pears in some localities (but not all) were hardly able to put forth any bloom, and the crops were consequently extremely poor. Wall-fruit was also a general failure, but this was partially owing to severe weather when the trees were in bloom, for in some instances the show of bloom was splendid. Gooseberries and currants produced enormous crops, and strawberries were very fine, but they lasted an unusually short time. Seeds generally ripened with difficulty; much of the corn could not be ground, and a great deal was mixed up with roughly-ground Indian corn and flavoured to induce the cattle to eat it. The crop of ordinary garden seeds was also far below its usual quality, and some of the favourite garden flowers were consequently very poor. Among the *special* features of the year may be mentioned the great quantity of certain insects. "Aphis" was in astonishing numbers in the early part of the year. The apple-shoots, before the leaves expanded, were in almost every case covered with the "green fly," and among wild plants the Mealy Guelder-rose was especially attacked by them. "Wasps," again, have been in extraordinary numbers, and dreadful accounts of them have been sent to the various entomological periodicals; their numbers appear to have exceeded all previous experience. The larvæ of the gooseberry moth and of the gooseberry saw-fly have also been extremely destructive; and finally, as an undoubted result of the wet season of 1879, the larvæ of the crane-fly have been a perfect plague in some localities, and sheep-licks in others. The scarcity of small birds has been universally noticed; some, no doubt, perished from the cold, but vast numbers had migrated. The enormous numbers of larks which hastened to the Eastern Counties on the outbreak of cold weather was astonishing.—On the variations of relative humidity and thermometric dryness of the air, with changes of barometric pressure at the Kew Observatory, by G. M. Whipple, B.Sc., F.R.A.S.—On the relative frequency of given heights of the barometer readings at the Kew Observatory during the ten years 1870-79, by G. M. Whipple, B.Sc., F.R.A.S.

**Mineralogical Society of Great Britain and Ireland,** December 23.—Prof. M. Forster Heddle, F.R.S.E., president, in the chair.—Prof. F. J. Wiik of Helsingfors was elected a corresponding member, and Messrs. Baxter, Gray, James Cunningham, R. Shaw Simpson, H. B. Guppy, and Stephen Vivian as ordinary members.—The following papers were read and discussed:—On Tyrceite, by the President.—On minerals new to Britain, by the President.—Note on Gilbertite, and on tin pseudomorphs from Belowda Mine, by J. H. Collins.—On Brochantite and its allies, by William Lemmons.—On a remarkably fine crystal of Euclase, by L. Guyot.—On the action of organic acids on minerals, by Prof. H. C. Bolton.—Note on artificial Gay-Lussite, by C. Rammelsberg.—Note on a peculiar carbonaceous substance from the Maesymarchog Colliery, by James S. Merry.

**Anthropological Institute,** December 14.—Edward B. Tylor, F.R.S., president, in the chair.—The election of the Rev. R. A. Bullen as a member of the Institute was announced.—Mr. W. St. Chad Boscawen read a paper on "Hittite Civilisation."

**Institution of Civil Engineers,** December 21.—W. H. Barlow, F.R.S., president, in the chair.—The scrutineers reported that the following gentlemen had been duly elected to fill the several offices in the Council for the ensuing year:—Mr. James Abernethy, president; Sir W. G. Armstrong, C.B., F.R.S., Sir J. W. Bazalgette, C.B., Mr. F. J. Bramwell, F.R.S., and Mr. J. Brunlees, vice-presidents; Mr. G. Berkley, Mr. G. B. Bruce, Sir John Coode, Mr. E. A. Cowper, Mr. A. Giles, Sir Charles A. Hartley, Mr. H. Hayter, Dr. W. Pole, F.R.S., Mr. R. Rawlinson, C.B., Mr. A. M. Rendel, Dr. C. W. Siemens, F.R.S., Mr. D. Stevenson, Sir W. Thomson, F.R.S., Sir Joseph Whitworth, Bart., F.R.S., and Mr. E. Woods, other members of Council.

EDINBURGH

**Royal Society,** December 20.—Sir Wyville Thomson, vice-president, in the chair.—Mr. John Aitken read a paper on dust, fogs, and clouds, which we give on another page.—Mr. E. Sang communicated a note on the solar eclipse of December 31, 1880, which is visible in our islands.—Dr. Marsden, in a paper on the preparation of adamantine carbon, intimated that he had at length effected the crystallisation of carbon in the cubical form. The crystals he had obtained were however far too minute to be of any commercial value.—Prof. Blyth described an electric sonometer, consisting of a wire monochord, which, traversed by an interrupted electric current, was set into strong vibrations between the poles of a horse-shoe magnet. The notes given out were loudest when they were harmonics of the fundamental interrupted note which was sounded by a vibrating tuning-fork inserted in the circuit—Dr. Haycraft communicated an explanation of the amœboid motions of masses of protoplasm, illustrating his theory by an extremely simple mechanical contrivance. An india-rubber ball perforated with several small apertures was filled with coloured white of egg, and immersed in a solution of sugar of about the same density as the albumen. When a gentle pressure was applied, the albumen was forced out in long continuous processes; and when the pressure was relaxed the processes at once retracted inside the ball again, probably in virtue of the action of the viscosity and surface-tension of the gelatinous matter. Thus was explained the retraction of the amœboid processes, after they had been expelled by contraction of the internal muscular structure.

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